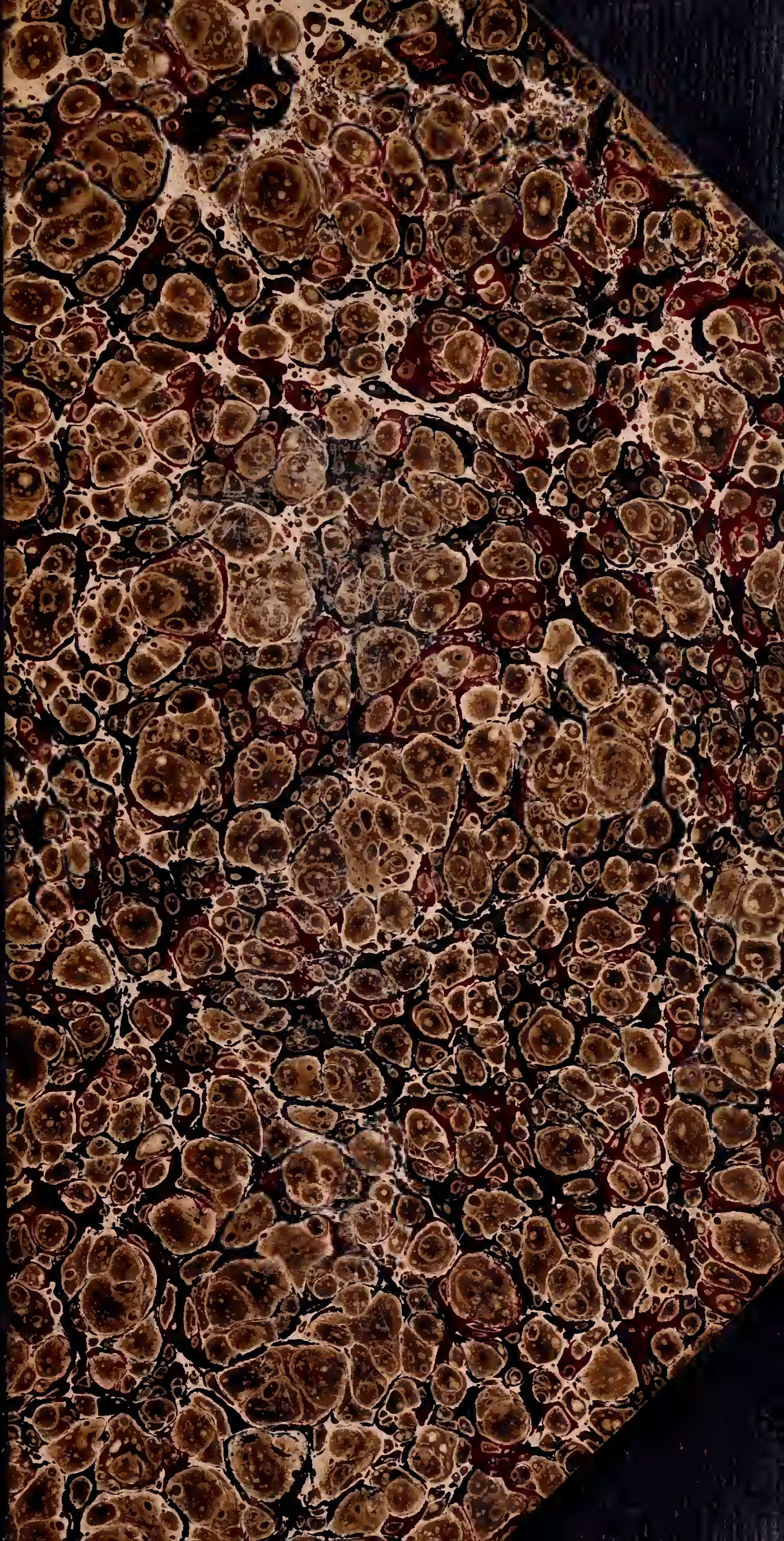

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GAILLARD'S MEDICAL JOURNAL.

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ORIGINAL ARTICLES.

THE STATUS OF SERUM-THERAPY, WITH ESPECIAL REFERENCE TO DIPHTHERIA ANTITOXIN, AS TYPICAL.

By EDGAR W. ROBERTSON, M.D.,
Onancock, Va.

In writing upon "The Status of Serum-Therapy," it must be seen that but little originality can obtain aside from digested thought, reflection, and arrangement of my article. That part of my article which has the greatest approach to originality, has been from notes dotted down at different times, mostly in riding to and from my patients; and while silently waiting at their bedsides, or room. And part of this must be in consequence of having my mind imbued with the points, etc., from reading results of clinicians, or their critics. Direct quotations, verbatim, as for extracts, or points gleaned as abstracts, have been acknowledged by marks, or name of authority, or both. After all, my greatest effort, perhaps, has been that of the industrious seeking after truth.

It might be still further fair to state that the articles to which I am indebted for research, are the following (besides some correspondence):

Brochure from Schulze-Berge and Koechl (New York, 1894-5).

New York Therapeutic Review (April, May, June, 1896.)

Gaillard's Medical Journal (different numbers, 1895-6).

The Sanitary Era (May, 1896).

The Journal of the American Medical Association (March 21, 1896), etc.

It is wise to know the character of our remedies. It would be well if both the character and standing were in harmony. Such is not always the case. My paper has reference to the standing ("Status"), of Serum-Therapy; which may lead up to an approximation to its real character. We judge of a remedy from personal experience, from accepting the testimony of others, or by a rational deduction from accepted premises. But our judgment may err in personal experience, or our deductions may be based upon untenable propositions, and the weight of reliable testimony may be meager. Waiving the first two sources as a foundation stone for our judgment, let us conceive of a remedy, or agent, of proposed therapeusis, toward which the eyes of the medical world, were turned, some its friends, some its foes, with a great number of fair-minded judges, who at the assize can deal fairly; and as report after report, article after article, appear, almost weekly, daily, we can form some judgment as to the agent. Such is the case with Serum-Therapy.

With this therapeutical lever I have had no experience, and, therefore, my paper must rest on testimony and rationality combined.

The power of the Blood-Serum-Therapy is the antitoxin, of which it is the carrier. And it might be well just here to ask what is antitoxin? Taking that of diphtheria as typical, from my understanding of the materia medica as it were of the blood-serum agent (digested from Krieger's article), it is simply the inoculation of beef-tea with the bacilli, incubation allowed to take place, toxin formed, animal injected and immunization resulting; blood drawn, coagula formed, serum separated, anti-toxin resulting (after adding a fraction of carbolic acid, or other manipulation), and being injected into mankind (genus-homo), becomes a therapeutical measure of a prophylactic, or curative nature.

Now, what have been laboratory results, upon lower animals, for instance, under strict official surveillance? I quote the following from the New York Therapeutic Review (April, May, June, 1896), verbatim:

"REPORT OF THE LABORATORY OF THE NEW YORK PASTEUR INSTITUTE ON ANTITOXINS.

"Experiments recently made with the Antitoxin of Diphtheria, Tetanus and Septicæmia, in the Laboratory of the Pasteur Institute, have given the following results:

"Diphtheria.—The injection to guinea-pigs of a quantity of this antitoxin (Gibier's), in the proportion of 1-150.000 of their weight, has enabled them to survive an injection of two drops of the virulent culture of diphtheria bacillus administered 24 hours later. One drop of this culture inoculated to the 'control animals' caused death in 24 hours.

"If the above figures are valued in immunizing units, according to the formula of Spronck, each cubic centimeter of this diphtheria antitoxin (Gibier's), contains 300 units. The horses from which the serum is taken have received during the last twenty months over 5,000 c.c. of diphtheria toxin, each horse receiving an average of two injections weekly.

"Tetanus.—The injection to guinea-pigs of a quantity of this antitoxin in a proportion of 1 to 1,000,000 of their weight, enabled them to survive a fatal dose of tetanus toxin. The same dose given to the 'control animals' caused death within 48 hours.

"Septicæmia (Erysipelas Streptococcus).—The injection of this antitoxin to rabbits, in a proportion of 1 to 10,000 of their weight, has enabled them to resist a dose of streptococcus culture fatal to 'control animals.'

"A. Leteve, M.D., Director of the Laboratory of Bacteriology.
"F. T. Labadie, M.D., Assistant, Hydrophobia Department.
"Geo. G. Rambaud, Assistant of Bacteriology.
"E. Esquerre, B.S., Assistant of Chemistry.
"J. Esquerre, B.A., Assistant of Bacteriology.
"Paul Gibier, M.D., President."

ITS EFFICIENCY.

I have compassed the universe, as it were, not to make one proselyte to Serum-Therapy, but to gain light upon the subject, from all climes. Now what are the results from sero-therapeusis? And why not commence at its birthplace—Germany?

Schulze-Berge & Koechl, of New York, have compiled a very elaborate brochure (up to 1894-5), of results from such observers as are now shown. "Statistics in Berlin.—Dr. Behring has recently published a comparison of the results of the treatment of diphtheria in two of the Berlin hospitals. In the Charite, where antitoxin was employed, there were 299 patients, with 53 deaths, or 17 7-10 per cent. In the Bethania Hospital, where antitoxin was excluded, there were 249 patients, with 112 deaths, or 45 per cent."

"Official statistics show that the mortality from diphtheria in the cities of Berlin, Dresden, Leipsic, Munich and Hamburg during the first half of 1895, was conspicuously lower, both absolutely and relatively, than during any year covered by the records, although the whole number of cases of the disease, in the City of Berlin, for instance, was larger, and the proportion of cases received into the hospitals was smaller."

The following are actual results, from individual observers, collected from the brochure (as abstracted):

Huebner.—"Among the 110 uncomplicated cases treated between the first and third days, there were but six deaths (5 4-10 per cent)."

Schroeder (City Hospital of Altona).—"Injected 63 cases of diphtheria, 8 of which died (12 69-100 per cent. mortality).

"The mortality of the true diphtheria cases was 10.71 per cent., as opposed to 29.41 per cent. for 1889, the lowest of the seven preceding years."

Vierodt.—"Mortality was then found to be 14 per cent., as against 67 to 37 per cent. in the preceding six years."

LEICHTENSTERN and WENDELSTADT.—"Prior to antitoxin, 30 9-10 per cent. Subsequently, by use of antitoxin, 'fell to 20 3-10 per cent.'

HEIM.—"Twenty-two per cent. versus 52 4-10 by other methods."

MONTI, of Vienna (British Medical Journal), "Reports that he has treated 25 cases of diphtheria with the antitoxin, with but a single death."

UNTERHOLZNER.—"25 8-10 per cent., with versus 66 6-10 per cent. 'without serum.'"

SEIZ (Constance).—"Twenty-seven cases treated with antitoxin, with a single death."

GANGHOFNER (in Kaiser Franz Josef Children's Hospital, Prague).—"With antitoxin 12 7-10 per cent.; without, 43 per cent."

SIEGEL (Olga Hospital, in Stuttgart).—"Prior to use of antitoxin, from 1889 to 1893, 40 1-10 per cent.; and 1894, up to October 4, when the antitoxin was first employed, the mortality reached 50 3-10 per cent; but 'in 100 cases treated from October 4, 1894, to January 1, 1895, with the antitoxin, the mortality was 12 per cent. and 20 3-10 per cent., respectively.'"

FURTH (Clinics at Freiburg).—"The opinion is expressed that the antitoxin is a specific in the treatment of diphtheria with which no other therapeutic measure is to be compared."

RUMPE.—"Reported the results obtained in the new General Hospital of Hamburg from the employment of Behring's antitoxin in the treatment of diphtheria. The complete statistics for several months showed that there had been 91 cases of diphtheria in the hospital, with 11 deaths 12 per cent)."

ESCHERICH.—"Has treated 51 cases of diphtheria with the antitoxin with 5 deaths (9 5-10 per cent.)."

GOUGUENHEIM (Hospital Lariboisiere).—"Prior to use of antitoxin deaths in adults, 10 37-100 per cent.; children, 57 5-10 per cent. 'Of 77 adults treated with the antitoxin, three died (3 9-10 per cent.), and of 48 children, 9 died (18 75-100 per cent.).'"

DREYFUS (Charite Hospital of Lyons).—"With antitoxin, mortality of only 19 29-100 per cent. 'In previous years the mortality of diphtheria has been 50 per cent.'"

LE GENDRE.—"Antitoxin in '16 cases diphtheria, with 2 deaths.'"

LEBRETON (Hospital des Enfants).—"242 cases of diphtheria, with 28 deaths (11 66-100 per cent.)."

MOISARD.—"Reported the employment of the antitoxin during the months of October and November, 1894, in 248 cases of diphtheria, with 34 deaths (14 71-100 per cent.)."

WASHBURN, GOODALL and CARD (in London).—"With antitoxin there were 14 deaths, or 19 4-10 per cent., the average mortality in the same hospital during 1893, and up to October 22, 1894, was 38 8-10 per cent. The mortality of the 724 cases immediately preceding the beginning of the antitoxin treatment 38 8-10 per cent. In the opinion of the writers the cases treated with antitoxin were of more than average severity."

Zabolotnui (Podolia, Russia).—"In 109 cases proved bacteriologically to be true diphtheria, injections of anti-diphtheritic serum were employed. The number of deaths was 14. The percentage death rate was, therefore, 12 8-10 per cent. Previously, under the old forms of treatment, the rate had been 48 per cent."

This brochure also says: "Dr. Louis Fisher, of New York, says that before the introduction of this remedy his mortality in diphtheria was about 55 per cent. Since his use of antitoxin his mortality has been reduced to 15 per cent., and he now declares that he would as soon think of treating intermittent fever without quinin, as to treat diphtheria without antitoxin."

Editorial, Journal American Medical Association, April 20, 1895.—"When Professor Virchow, repudiating the attack of his assistant, Dr. Hansemann, on Behring's theory of the serum treatment of diphtheria, said: 'All theoretical considerations must give way to the brute force of these figures,' referring to the statistics of the treatment in the Kaiser and Kaiserin Friedrich Hospital, of Berlin, the question seemed to be settled as to the general merits of sero-therapy. During a given period 533 cases of diphtheria were treated in this hospital. Of these 303 were treated with serum, with a mortality rate of 13 2-10 per cent.; 230 were treated without serum, with a mortality rate of 47 8-10 per cent. A still more striking exhibit was afforded during June and July, when nearly all cases were treated with the serum, and the mortality rate was 12 7-10 per cent. 'Suddenly,' says Virchow, "the supply of serum ceased, as, unfortunately, the very horses from which the serum was taken died. The old methods of treatment had again to be resorted to and the results were: 109 cases, 55 deaths in seven weeks, a mortality rate of 50.46 per cent.'

"A supply of serum was then obtained from another source, and during the next six weeks 81 cases were treated with 12 deaths, a mortality rate of 14.81 per cent. It was the 'brute force of these figures' which Virchow said compelled him to recommend the serum, to attribute to it 'a brilliant therapeutic virtue;' to contend that, even if disagreeable by-effects were proved to occur occasionally, they were not sufficient to deter him from continuing the treatment."

* * *

Copy following from Gaillard's Medical Journal, November, 1895, page 286, as selected from British Medical Journal:

DIPHTHERIA ANTITOXIN.

"Dr. M. Charteris, professor of materia medica and therapeutics, University of Glasgow, said at the last meeting of the British Medical Association:

" 'I had the opportunities of observing, during the last winter, cases of diphtheria at Belvidere Hospital, which were treated by antitoxin. There seemed to me to be no doubt about its efficacy. The patches of membrane ceased to spread, and shriveled up after the injections. The ashy color of the face was replaced by a healthy hue and the pulse became fuller and stronger.' "

Coming to our own State: E. C. Levy, M.D., Richmond, Va., writing under subject of "Serum-Therapy of Diphtheria" (Medical Semi-Monthly, May, 1896, and from schedule of his data as member of Medical Society of Virginia), says substantially: "Klebs-Lœffler bacilli excite local trouble, and growing, develop diphtheria toxin, poisoning general system, and probably prove mortal but for a recoil, and 'antitoxin developed,' antagonizing further dire results.* The inference is, antitoxin artificially produced and applied has better chance for success.

* * *

Copy following from " selections " in Gaillard's Medical Journal, June, 1896, page 351. Author, Dr. J. M. French, of Massachusetts. Subject: "A Year of Antitoxin."

"Taking all cases reported together, the practical result of the first year's use of antitoxin, so far as can be judged at the present time, has been to lessen the death rate from diphtheria in cases where it has been used, nearly or quite one-half, thus proving itself beyond all doubt to be the most successful of any known treatment for this dread disease."

* * *

I have corresponded with some in the profession, among the number Dr. Wm. Osler, of Johns Hopkins. The following is his reply:

"Baltimore, May 28, 1896.

"Dear Dr. ——: You should read Professor Welch's paper on the 'Antitoxin Treatment of Diphtheria.' I will ask him to send it to you.

"I have just returned from Montreal at the Pediatric Society. The various members made a report on the serum treatment in private practice, which, in a total of some 5,000 cases gave the most remarkable results, very much superior to anything that has ever been obtained before.

"Very Truly Yours, Wm. Osler."

---

* Where the conditions are favorable.

THE JOURNAL OF EXPERIMENTAL MEDICINE.

"Baltimore, July 6, 1896.

"DEAR DR. ——: I have mailed you to-day a reprint of my article on the treatment of diphtheria by antitoxin. The reason that I have not done so before is that all of my reprints were exhausted, as well as the edition of the Hospital Bulletin containing the paper. I have, however, secured a reprint, which you can retain. I regret the delay in complying with your request. Very Truly Yours,

"WILLIAM H. WELCH."

"The treatment of Diphtheria by Antitoxin," by William H. Welch, M.D., professor of pathology, Johns Hopkins University of Baltimore.

The following is an abstracted historical sketch, in part, with some special points and a few extracts, winding up with his conclusions on his last page.

Dr. Welch gives Behring credit for the first conception of serum-therapy in 1890, showing the immunizing and curative quality of blood and its serum of artificially immunized animals for tetanus, diphtheria being implied, which was publicly announced in London in August, 1891; following this, in 1892, Behring & Wernicke published an article, in which these experiments were fully described, and which sets forth the fundamental principles underlying serum-therapy of diphtheria: "The first trial of immune serum in the treatment of human diphtheria was made in von Bergmann's clinic, in Berlin, in the autumn of 1891. This trial, together with those made in 1892 by Henoch in Berlin, by Heuber in Leipzig, and in the Institute for Infectious Diseases in Berlin, were of a tentative nature and made with weak serum and insufficient doses.

"It was not until early in 1893 that Behring succeeded in obtaining anti-diphtheritic serum equaling the strength of even his so-called normal serum, of which sixty times the strength is that of the weakest Behring serum at present in use. In April, 1893, Behring referred to thirty cases treated with this normal serum.

"From this period on Behring & Ehrlich succeeded in obtaining healing serum of greater and greater strength, until in August, 1893, Ehrlich and Wasserman obtained from goats healing serum twenty to sixty times the strength of Behring's normal serum."

* * *

The era of serum treatment of human diphtheria by approximately sufficient doses of antitoxin really begins with this publication of Ehrlich, Kossel and Wasserman in April, 1894:

* * *

"It is evident from this brief historical summary that the general principles of serum-therapy of diphtheria were fully established, and its application to human beings in active operation before Roux delivered his memorable address on the subject at the Eighth International Congress of Hygiene and Demography, held in Budapest, September 1-9, 1894, three years after Behring's original communication to the preceding congress in London. Roux, however, presented the subject with such clearness and force, and with such an array of convincing and carefully analyzed statistical evidence, that the attention of the great body of physicians throughout the world, who had paid little heed to the previous work, was arrested, and the question of the healing power of diphtheria antitoxin became and has continued to be the foremost medical question of the day."

* * *

Having gotten such statistical data from other sources will omit this from Dr. Welch's paper, and simply content myself with quoting from him one very important one, as follows:

"A most convincing demonstration of the healing power of antitoxin is furnished by the experience of Baginsky during an involuntary pause in the serum treatment caused by failure in the supply of serum. Between March 15, 1894, and March 15, 1895, there were treated in Baginsky's service by antitoxin 525 children, with a fatality of 15.6 per cent. During the period of forced interruption the serum treatment, this period being chiefly the months of August and September, 126 children were treated without antitoxin, with a fatality of 48.4 per cent. There was absolutely no selection of cases in either group. In his comments upon this experience Baginsky says:

" 'It is all the more remarkable as the ratio of mortality of those treated with the serum, both before and after the period of interruption, varied within very small percentage figures. If one will permit figures to speak at all, there has scarcely been made on human beings a more demonstrative test of the curative power of a therapeutic agent. It was an experiment forced upon us, but it proved to us how terrible was the form of disease which we were treating, and how numerous would have been the victims without the use of the healing serum.' "

* * *

"Dosage and timely administration are factors of prime importance in determining the efficacy of antitoxic treatment."

He thinks proper dosage can be obtained with more facility for diphtheria than for tetanus, which, however, must be after all "empirical," even for diphtheria; age of patient, and duration and severity of case are points for judgment to be exercised upon, and because of expensiveness and liabilities, better use inexcessive doses, but repeated.

Disclaims knowing much of the nature of antitoxin, or of the modus operandi, but says this is no argument against using therapeutically the antitoxin. He's inclined to accept the "vital theory" of its action, viz: "That the antitoxin acts through the agency of the living body, and probably in the sense that it renders the cells tolerant of the toxin." Animal experimentation and clinical experience teach early use of antitoxin.

Dr. Welch is right, and for two reasons of my own experience, or earlier thoughts, as follows:

Riding on an electric car in the streets of Richmond, 1888, the conductor confessed he didn't understand it, but could work it. When thirty years ago I first began to practice medicine I didn't exactly know the modus operandi of quinin. And who would have dared not to have used it, in the malignant ague? Before 1881 did the bacteriologist know all about it? Does everybody accept the Binz-Laveran theory (Bartholow, p. 196)? And yet we rely on the remedy—relied on it—and justly, do we—did we!

Sanctions use of antitoxin from a "clinical diagnosis" and then can go on with bacteriological process.

He seems confident that "Lœffler bacillus is the cause of diphtheria." The only prominent opposer of this view is Hanseman, but well met by Frænkel.

Dr. Welch admits experimental diphtheria more amenable to favorable results than clinical ("human") diphtheria, because the former is less apt to be contaminated or complicated with "streptococcus pyogenes," a large portion of fatal cases showing this element.

"Without doubt the remedial rôle of diphtheria antitoxin is materially restricted by its inability to combat developed streptococcus sepsis, broncho-pneumonia, and other complications referable to secondary infections, or to stop impending suffocation by immediate removal of mechanical obstacles in the form of false membranes in the air-passages; but the antitoxic serum is the most powerful agent which we possess to prevent the development of these complications and secondary infections."

"So far, then, as the testimony of physicians, based upon their clinical experience is concerned, this, as I have already said, is overwhelmingly in favor of the antitoxic treatment, wherever their experience in its employment has been a large one. Those with less experience are often even more enthusiastic; but many of these, in view of their limited experience, are wisely conservative and a few are hostile to the new treatment."

* * *

"These occasional untoward effects of the healing serum are annoying, but, being unattended with danger to life and without serious consequences, they do not contra-indicate the use of the serum.

"There have been a few cases reported in which the writers, without any satisfactory evidence whatever, have referred the death of the patient to the use of the serum. The essential harmlessness of the serum has been demonstrated by over a hundred thousand injections, and if future investigations should show that through some idiosyncrasy on the part of the patient, death ever is attributable to the injection of the serum, this would probably count for about as much as the rare deaths from the use of ether or chloroform. I shall leave untouched the question of the immunizing properties of antitoxin.

"The principal conclusion which I would draw from this paper is that our study of the results of the treatment of over 7,000 cases of diphtheria by antitoxin demonstrate beyond all reasonable doubt that antidiphtheric serum is a specific curative agent for diphtheria, surpassing in its efficacy all other known methods of treatment for this disease. It is the duty of the physician to use it.

"The later reports show in general a decided improvement in the results of the treatment over the earlier ones, and there is every reason to believe that the results of the second year's employment of the new treatment will make a much more favorable showing than those of the first year. We shall come to a clearer understanding of the mode of action of the healing serum. Improvements in the methods of preparation and preservation of the serum, and possibly the separation of the healing substance, at least from other ingredients which produce the undesired effects, may be expected.

"The discovery of the healing serum is entirely the result of laboratory work. It is an outcome of the studies of immunity. In no sense was the discovery an accidental one. Every step leading to it can be traced, and every step was taken with a definite purpose and to solve a definite problem.

"These studies and the resulting discoveries mark an epoch in the history of medicine. It should be forcibly brought home to those whose philosophic sentiments outweigh sentiments of true philanthropy that these discoveries, which have led to the saving of untold thousands of human lives, have been gained by the sacrifice of the lives of thousands of animals, and by no possibility could have been made without experimentation upon animals."

The following from the Bulletin of the Maryland University Hospital, Baltimore, Md., June 1, 1896; by J. E. Atkinson, M.D., professor of materia and therapeutics, clinical medicine and dermatology, University of Maryland. Subject, "The Present Status of Therapeutics," is so full of truth I may be excused for copying:

"In 1796 Jenner first practiced vaccination against smallpox. It was a marvelous, glorious discovery of incalculable value to mankind and sheds an undying fame upon its discoverer. It resulted from the accidental observation of phenomena by a mind great enough to understand their significance. It was a purely empirical discovery, and such it remains to-day. Almost a century later the antitoxin of diphtheria was discovered, but under what different conditions! Conditions that in Jenner's time were impossible, were impossible, indeed, until within a decade. Purely scientific inductions led to the discovery, only less important than that of Jenner, but still in a measure empirical, for the exact nature and the methods of its operation remain unknown as yet, though there is little doubt that the new therapeutic and prophylactic procedure will be understood before long.

"Thus has the empiricism of to-day, by marvelous subtlety of reasoning and of scientific research, discovered that the animal organism is capable, not only of building up bulwarks against the enemies that wage incessant war against it, but of creating weapons of offense and munitions of war with which they may be attacked and routed. Henceforward, the vis medicatrix naturæ (an old phrase with a new meaning), will be harnessed to the car of the therapeutist, and from the body itself will be derived the most potent of the remedies with which to destroy the disease which assails it. What has been accomplished in the treatment of hydrophobia, tetanus and other animal diseases, but above all in the prevention and cure of diphtheria, is an indication of what is yet to be achieved in the treatment of specific disease, and is the noblest triumph of the therapeutic art."

* * *

Dr. S. C. Chew, of the University of Maryland, kindly gives me his opinion as concerns diphtheria toxin. Here is his letter to show. Have crossed out that part of which there is no need of printing.

Baltimore, September 22, 1896. E. W. R.

"Dr. E. W. Robertson:—

"Dear Doctor: I have had no personal experience with serum-therapy, except in the treatment of diphtheria, the results of which are no longer questionable and are common property of the profession. I think that there is good reason for believing that serum-therapy will make great advances in the next few years, and will be productive of most important results. Very truly yours,

"S. C. Chew."

* * *

Its march is onward, and its fate will be different, too, from the brave six hundred at Balaklava, for I feel sure its final victory will be certain. For, as a quiet worker in silent night the "phagocytic function" of leucocytes may be going on for cure (which I have thought may be mere theory), yet we know that vaccination is a tangible weapon, and antitoxin almost equally as conspicuous in its effects, though as it were seen "through a glass darkly;" but with less prejudice, more light, more time, it is reasonable to suppose victory will be heralded! Already many, many battles have been, and are being won against death, yet "the war is not over."

HOW USED?

In reading this point occurs: Rectal injections of antitoxin to cure, or toxins to immunize (an animal), futile. See New York Therapeutic Review (April, May, June, 1896). Article: "The Effect Produced Upon Certain Animals by Injecting in the Rectum the Toxins and Antitoxins of Diphtheria and Tetanus;" a paper communicated to the Academi des Sciences, of Paris, March 30, 1896.

En résumé from these experiments we may conclude that:

1. "In rabbits, dogs and guinea pigs the rectal injections of relatively massive doses of diphtheria and tetanus toxins are followed by no apparent effects.

2. "The rectal injections of even large doses of these toxins, many times repeated, does not produce the least degree of immunity against the toxin thus injected.

3. "The rectal injection of doses of antitoxin (diphtheria and tetanus), at least a thousand times larger than the preventive subcutaneous dose, is powerless to prevent death from a minimum fatal dose of diphtheria or tetanus toxin.

4. "The rectal mucous membrane may retain the active principles of the toxins and the antitoxins, or it may destroy them. In case it permits their absorption, the inference is indubitable that they are carried through the portal system to the liver and destroyed by that organ."

In a note, however, it is said:

"It is a matter which is at present under experimental study in the laboratory of the New York Pasteur Institute."

Conclusion: The process generally adopted, I find, is hypodermic method, and "the preferable place for the injection is on the lateral part of the chest, below the axilla where, on account of the soft subcutaneous tissue, even large quantities are absorbed. The application is made with the usual aseptic precautions." (Krieger, in Brochure, quoted formerly).

According to Edwin Rosenthal, M.D., Philadelphia, Pa., (Brochure):

"As to the mode of using. My injections were all made in the back, to the one side or other of the spine, under the scapula. The parts were previously washed with alcohol, soaked upon sublimate cotton, and after injection the parts were sealed with iodoform-collodion."

MODUS OPERANDI.

One would rationally suppose there would be an out and out fight with the bacillus of any given disease, from its name, namely, antitoxin, against the poison. But such is not very pointedly accepted, but there seems to be an indirect battle, in simple strengthening of the fort and supplies, ready for the siege; until the enemy himself grows weary and starves.

Copy of the following from Gaillard's Medical Journal, January, 1896, p. 27 (selected):

"THE NATURE OF IMMUNITY."

"In a lecture delivered before the International Congress of Hygiene, Professor Buchner, of Munich, summarized his conclusions in regard to immunity and immunization. The action of the antitoxins is not directly upon bacterial products, but by modifying the organism, these enable it to resist specific poisons. This is essentially the doctrine most prevalent."—Physician and Surgeon.

Following is an extract from a selection from Medical and Surgical Reporter, by GAILLARD'S MEDICAL JOURNAL, June, 1896:

"But now a new path was blazed in the wilderness of bacteriotherapy. We owe to many observers, but notably to Roux and Behring, the discovery of the fact that there are formed in the blood of persons recovering from the specific infectious diseases, substances which antagonize the poison of the disease, and thus lead the way to recovery. Further investigation showed that if the blood-serum containing the substances be injected into the blood of subjects suffering from a disease its course is thereby arrested and a cure effected, provided the constitutional effects have not proceeded too far, and also that a similar injection in healthy subjects confers a temporary immunity from the disease. Whether this result is due to chemical or vital action is not yet definitely settled, but the weight of evidence decidedly sustains the vital theory, namely, that the antitoxin acts through the agency of the living body, and probably by rendering the cells tolerant of the poison."

And this is in accordance with Welch. (See another section where I have quoted.)

* * *

Dangers as compared with efficiency, or, pro and con, together with the natural history of diphtheria, etc., considered.

Idiosyncrasies must be expected, for they arise from mercury, opium, chloroform, and why should this agent be exempt? It appears, unless it can be disproven, that an injection of antitoxin may possibly cause death in a case from shock (?) or otherwise.

A schedule, or synopsis, from the following authors or clinicians, and from the same sources in part, as formerly confessed; this being specially in reference to the clinical history, or effects of antitoxin. This in reference to antitoxin, and after this will follow clinical history of diphtheria itself, and comparison made, or conclusions drawn or intimated.

Krieger, Gordon, Schroder, Vierordt and Muehlec, in the Brochure; Levy, in Virginia Medical Semi-Monthly, and Dr. William Welch, in special pamphlet, harmonize in rash being common. Schroder and Gordon, as for albuminuria, temporarily; but Vierordt says less frequently. Gordon says may be renal irritation, while Schroder says nephritis in only six cases in a statistic of forty-four; and Welch thinks evidence shows less nephritis in new treatment; though albuminuria, probably no less common, the opinion of Kossel, Roux, etc., to the contrary notwithstanding. He says: "It is simple albuminuria, with, perhaps, a few narrow hyaline casts, but without evidence of any serious damage to the kidney," but further says: "Albuminuria is such an extremely common symptom of diphtheria that it must be very difficult to determine that it can be referred to the serum in any case."

He quotes Baginsky as saying minor heart troubles fully as frequent, but the major heart troubles favorably influenced by antitoxic treatment. Regards effects from antitoxin "annoying" sometimes, but not dangerous, and evidence of death therefrom insufficient.

The Sanitary Era mentions several deaths from antitoxin injections. Quote the following from Dr. Welch as to good effects:

"Antitoxic serum is a new and strange remedy, but the effects which follow its injection in individual cases are not new and strange. Nothing happens which the physician may not have occasionally seen to happen in cases treated in the ordinary way. In severe, as well as in mild cases of diphtheria, he may have seen an apparently progressive local process quickly arrested and the general symptoms promptly abated. Why should anything new and strange happen after the administration of antitoxin? Cure by antitoxin is cure by Nature's own remedial agent. That which is new and strange is the frequency with which in case after case the timely injection of antitoxin promptly arrests the local inflammation and checks the constitutional disturbance."

Now, in reference to certain symptoms, let us consider are they sequel to, or complicate diphtheria, entering into its clinical history, aside from antitoxin treatment, or from antitoxin? That these symptoms have been present in diphtheria before use of antitoxin, following testimony (before birth of antitoxin or its being known) shows:

Pharyngeal or Laryngeal Paralysis.—(U. S. Dis., 17th Edition, p. 718).

Local or General Paralysis.—(Da Costa, "Medical Diagnosis").

Cardiac enfeeblement even up to death; and paralysis, local and general.—(Day, "Diseases of Children," p. 343, 1881).

Death from heart-clot or paralysis. May have local throat or limb paralysis.—(Eustace Smith, "Disease in Children," pp. 99-100, 1884).

Albuminuria and casts.—(Flint's "Clinical Med.," p. 106, 1879).

Paralytic phenomena, which, if owing to trouble at medulla oblongata or fourth ventricle, may end fatally. May die of dysphagia; foreign substance entering larynx; failure of lung function, or hearts, or syncope; or from convulsion, or coma.—(Bristowe's "Theory and Practice of Medicine," 1879, pp. 206-7).

Paralysis (following), or nephritis (complicating).—(Goodwin's "Treatment of Diseases of Children," pp. 49-50).

Therefore, cannot part of the apparent havoc charged to antitoxin be explained away by natural history of the disease—diphtheria?

It has been charged that the horse-serum's "globucidal character" is the explanation. This seems refuted by the following. (See Philadelphia Therapeutic Gazette (Hare & Martin), June 15, 1896, (disproves injuries from serum itself):

"HORSE'S SERUM AS A GENERAL TONIC."

"Lacruz, of Barcelona (Arch. de Gin. Ostet. y Pediatria, December 25, 1895), has tried the effect of serum drawn from a healthy horse on several children in his wards. Doses of three to five cubic centimeters were injected daily for a period of three or four weeks. The serum seemed to act as a most powerful tonic. The red corpuscles increased in number, weight was gained and the children became visibly stronger. There was no untoward secondary effect, except slight rise of temperature and some acceleration of pulse; neither was there any erythema or albuminuria. A trifling epithelial deposit seen in the urine indicated slight irritation of the kidney, and the urine became more acid. These phenomena, however, were merely transient. The good effects of the injections were quickly manifested; cases of athrepsia, in particular, were speedily benefited, and the cure was maintained. Sixteen cases of chorea treated in the manner described were cured in a period of fifteen days on the average.—British Medical Journal, March 7, 1896."

* * *

There is one point which I've inclined to—a ptomain possibility. I thought one night some weeks ago about Austin Flint's article in the Forum (191) about "The Revolution of Medicine" and Koch's lymph that toxic bacilli effect through toxic ptomain—and then this idea of ptomain, in reference to antitoxin, was born. And last night, while on my bed, awake at midnight, this thought occurred (and I was not aware I'd seen it expressed before), that herein might rest the secret of the danger of stale antitoxin, and I got up and noted it down.

* * *

The Sanitary Era, in its article "Dealings With the Devil," writes (May, 1896):

"To the sudden deaths following diphtheritic serum injections in this city and Brooklyn closely follows the intense disturbance in Berlin, caused by the death of the little son of Professor Langerhaus, medical director of the Moabit Hospital, from an inoculation with Behring's serum. The report is that the little fellow, though perfectly well, was inoculated to prevent infection with the disease from a servant; and that, though the greatest care was taken, he died within two minutes. In the absence of professional details or corrections, we do not comment on this case further than to repeat the sentiment we expressed on the first occasion of the kind, the killing of Miss Valentine, of Brooklyn, by the same agent, to the effect that the event should command a halt, at least until the secret of so terrible a disaster from the use of a new remedy could be ascertained and eliminated from the treatment. (It has since, by autopsy, been determined, that death was due to foreign body, from vomited matter lodged in trachea").

* * *

"By a late news item it is learned from Washington that Ambassador Uhl, in his first cablegram to the State Department since reaching Berlin, informs the department that the German Government has prohibited the manufacture and sale of 'Control No. 40,' the serum manufactured for the cure of diphtheria."

The following from Gaillard's Medical Journal (editorial), June, 1896, can equally offset. "As to whether he has reference to the Era, I know not, but I place one against the other and let them "break lances together"):

"It has been said that statistics prove everything or nothing. We are not disposed to accept either statement, but we cannot refrain from observing that the records of the last eighteen months have shown throughout the whole civilized world such a material decrease in the percentage of deaths from diphtheria where antitoxin has been used, that the evidence of its value cannot be neglected, however much it may be questioned. Only last week one of the best known practitioners in New York city, who has given special attention to this subject, both in this country and abroad, made a most bitter attack upon antitoxin as a therapeutic agent, the same writer having taken the same stand early in the days of antitoxin therapeusis. There were several others at this meeting who gave their support to the speaker's views, and the question, therefore, becomes again one for earnest consideration and discussion.

"We have always believed that the large quantities of antitoxin necessary for immunization and treatment would sooner or later be materially reduced, and with this reduction would come a like reduction in the injury to the blood corpuscles. Indeed, this has already been done, for there is hardly a laboratory from which antitoxin can be procured, at which is not obtainable in different strengths. The advisability of its use for immunity is open to more question, in our opinion, than its value in the disease. The best results have, with a fair degree of uniformity, been obtained when the serum was used early in the affection, and when there might be a question as to the diagnosis; but it must be remembered that the statistics embrace a great many hundreds of cases in which, if bacteriological examinations are to be depended upon, true diphtheria was already an assured fact. Unquestionably, the personal equation will enter largely into its use, unless the public arrive at a condition where a failure to use it will be regarded as criminal negligence. With this, as with many other new remedies, its fate for each man is often determined by the results in a single case, without reference either to the quality of the material used, or the character, stage and condition of the case upon which it is employed. Those who have been unfortunate enough to have a paralysis, sudden death or a severe nephritis co-incident with, or sequent to the use of antitoxin, will readily join in the crusade against it. Those who have seen cases in which a material and rapid improvement seemed to follow its use—cases like those which, in their experience, have previously proved fatal with the ordinary means of treatment—will doubtless be eager to rush to its defense. The present status of the question undoubtedly justifies the early use in moderate quantities of a good preparation of antitoxin. We are not prepared to say that the failure to use it under such conditions would be as easy to explain or to justify."

* * *

Is it an impossibility (or extremely unlikely) for even an experienced hypodermist to have injected air into a vein, with shock or death resulting?

No doubt there's a lion in the land. Whether he can be tamed and be made a safe burden-bearer, able, capable, reliable to such a degree that an overwhelming tendency for good be conceded by all, with rare exceptions, is the question for time to prove. Electricity has been "subdued" for our purposes, but with liabilities to accidents.

There's a growing evidence in favor of antitoxin treatment, which is a little short of being established beyond questioning. At present too much may be claimed for it by its friends; too little (for good) admitted by its foes. The compromise, or place of meeting will probably be somewhere between the extremes, or antithesis of the rod, but even now is nearer the affirmative than negative end. The strongest tide is in favor of antitoxin's use. The spirit manifested by its advocates is fully as conservative and consistent as the opponents, and as for many, fully as disinterested, personally. That general excels who can gain vantage ground with the least loss of life, though he may lose his child in battle. From somewhere and at some time I learned this formula: The least of that which is the greater is greater than the greatest of that which is least.

Now, here is the application: Failures, and even some accidents, with the remedy, when used for good purposes and conscientiously, are better than the "hands off" of those who, though may not take any risks, fail in probably a greater number of cures than if they had practised serum-therapy.

MY CONCLUSIONS.

A digest, or synopsis, of my study, reading, reflection, thought:

1. Antitoxin is a therapeutical measure of importance.

2. It is powerful for good in the great majority of cases, especially if used in the incipiency of the case.

3. "Delay is dangerous," for in proportion as the system is saturated with the disease germs, in same rate are chances of victory for antitoxin lessened. The great need in the proposition is this: Permeate the tissues and blood with antitoxin early enough to assist the recuperative powers of nature.

4. Wiser to use a moderate dose as curative and minimum as preventive. But dosage not positively agreed upon. One party favors large doses—even larger for a child—to destroy the powerful germs (or antagonize); other smaller doses, so as not to endanger disorganization of blood globules. The requirement is proper dosage, but most evident that minimum is preferable to maximum as already intimated, for this reason: Intact red blood globules are more important (admitting their liability to injury by serum) to the economy, than deficiency of antitoxin, especially when used early. Besides, antitoxin can be repeated, upon this principle, viz.: Powerful remedies can be borne better in small, repeated doses, than in one large one. This in accordance with established principles of therapeusis generally.

5. Recent antitoxin safer.

6. There never was a drug of power that has not shown some untoward results, for some reasons; or that, at times, has not utterly failed.

7. It is agreed that the hypodermatic method is the plan. Antitoxin inert (or without effect) per rectum, unless through a broken surface.

8. Doubt about the diagnosis of diphtheria, or bacteriological results not imperative (at least need not delay treatment), and need not forbid the use of antitoxin. Proof: It is used to immunize those liable to be attacked, and, if so, why not to counteract supposed poison already existing?

9. I conclude this question as yet unsettled, namely, whether it is an individual practitioner's "bounden duty" to practice sero-therapeusis. I believe the moral code at present, while there is questioning among ad-

mitted competent judges, must decide for him from an individual standpoint:

"The lesser doubt must yield to the greater."

An experience: Last year I had a case of empyema, with pent-up pus, fever and diarrhœa when I saw him. I irrigated with reduction of fever. The treatment was corroborated by one of our best surgeons, in letter of reply to me: "He would have been dead ere this time if you had not drained him and washed out the cavity." One day in irrigating to bring out the pus he was attacked (during the process) with tetanic convulsions, and for several days was near dying. Afterwards, in this same case, it seemed best to two of us, with the consent of the family to irrigate again, which was done, and repeatedly afterwards, being careful, however, to let the fluid flow out after nearly every ounce introduced. This same surgeon kindly replied to another letter of inquiry thus:

"DEAR DR. ——: Yours of Tuesday received. The tetanic convulsions may have resulted from the washing out, a certain number of such instances are reported, and it would be therefore unwise to continue the irrigation. It would seem to be more safe if the tube were retained still, and the flow from the cavity allowed to take place freely. If you have had the tube out some days and no evil has followed, you will, of course, defer re-introducing it until circumstances justify.

With kind regards, yours truly, —— ———.

I mention this, and it is applicable to my above conclusion: If in these days of sepsis in surgery, a measure of antisepsis be questionable, much more open to question is a method as yet not unanimously received by the profession, and one man may have clear conscience in using, and another in not using serum-therapy.

THE BRAND METHOD FOR TYPHOID FEVER IN PRIVATE PRACTICE.

By CHRISTIAN SIHLER, M.D., Cleveland, O.

The communications of Osler and of Wilson on the hydriatic treatment of typhoid fever in hospitals induce me to add a few words on this mode of treatment in private practice.

That Osler and Wilson will save the lives of hundreds upon hundreds of typhoid fever patients and bring comfort and relief to thousands, I, for one, feel certain. They will accomplish this principally by instructing the coming generation of physicians in the use of the bath-tub and making the American people acquainted with the Brand method. They can do this to a less degree by lowering the mortality from typhoid fever in the hospitals, in which they can make their influence felt.

While I am of the opinion that the Brand method ought to be the regular form of treatment, I would not think of reproaching any one for not using this method systematically in general hospitals. The baths act in some sense like the antiseptic method in surgery: They do good by preventing the harm the disease would otherwise do, but they cannot undo the harm brought about by the disease in one or two weeks. The amount of good which can be accomplished, where advanced cases are subjected to the bath treatment, is rather meager, when the amount of labor is taken into consideration, that has to be spent on them. Necessarily in a number of such cases labor must be wasted. Nor should we judge of the value of this method by results obtained on cases ill one to two weeks, any more than of the value of antiseptic surgery by its effect on wounds of two or three days' standing. On the other hand, I do not see why hospitals should exclude late or even moribund cases from their statistics as long as they practice medicinal treatment, because the treatment which these cases received at home was essentially the same as that which they would have received in the hospitals.

Some years ago I published in the Medical News a paper on the hydriatic treatment of typhoid fever in private practice, and expressed myself in favor of the method.

Nothing, I think, will interfere so much with the introduction of a new method as the suspicion in the minds of the profession that the friends of the new method may be uncritical enthusiasts who are given to report the favorable results only of the new line of treatment they are advocating. As I still wish to be regarded as an advocate of the Brand method, I would state that during the years of 1893 and '94, out of a small number of patients I lost not less than three persons from typhoid fever and its sequelæ. The immediate cause of death in all three cases was pneumonia.

I expect to hear now the outcry: Was it not predicted that the cool baths would bring on pneumonia?

I can, however, in a measure, quiet the apprehensions of these objectors.

In the first case the patient had been improving a number of days, perspiring freely, and the baths had been omitted, when one morning instead of a normal temperature I found a high fever; delirium and a typical croupous pneumonia spreading over one whole lung and carrying off my patient on the seventh day. While being bathed neither bronchitis nor cough had been present. In the second case, also, pneumonia set in after we had omitted the baths, partly on account of a hæmorrhage, partly on account of improvement in the general condition. In the third case alone pneumonia set in while the baths were being used.

That pneumonia is a so-called complication of typhoid fever has been known long before baths were used. In fact, since using the baths I was called to treat cases of typhoid fever where pneumonia had been developed, without the patient having been bathed, and one of my colleagues here, Dr. L., informed me lately that he had apparently saved the life of a grave case of typhoid complicated with pneumonia, and about given up by the physician who treated without water, by the use of the baths.

Looking upon pneumonia as an infectious disease, I do not see why cold should produce it any more than typhoid fever, and as in my experience the cool baths act more like a tonic than any therapeutic procedure or medicine I am acquainted with, I have no reasons to think that the pneumonias in the cases mentioned had anything to do with the baths. Having never been of the opinion that one could save every case of typhoid by the baths, this experience has not made any change in my opinion on the value of the baths.

To illustrate the potency of the baths I will relate a case, which, in a glaring way, demonstrated the valuable properties of the water treatment. The patient was a young lady, a teacher in the public schools, ill over a week. I found her in a delirium of the most intense kind, with all the muscles of the trunk in a semi-pasueodic condition, so that the body was almost rigid. With one arm she was carrying out convulsive movement. Asked to show her tongue, she stared at me with a stupid expression, and clenched her teeth. In fact her teeth were set so firmly all the time that the nurse was unable to get her to take fluids; urine and fæces were discharged involuntarily, and there was a bed-sore of the size of a dollar, although the disease had not lasted much over a week. Her temperature was 103, her pulse from 130 to 140. She had not slept for a week, and had been in the condition I found her for three days. The fact alone that fluids could not be given made the case a grave one in my opinion.

Now, as to the treatment. I do not consider it wise to begin with the regular formula in a case like this one was, even if serious organic changes are absent. (Thus, in a case with many unfavorable features, which I saw in consultation lately, baths of 84° produced at first very marked depressions of temperature, from 105 to 102. This, in my opinion, was a bad sign. After a few days bathing the temperature was more stubborn, and we then lowered the temperature of the baths.) In such cases I think it is well to use the luke-warm half-bath with cold affusions, a bath principally stimulating, which at the same time allows one to form an opinion as to the cooling which the patient will bear.

My directions in this case then were to give a bath of 90° and ten minutes' duration, and to use three pails of water from the hydrant for affusion to the head. The baths were to be repeated every two hours, excepting the patient should be sleeping, when three hours should be the interval. Of course the hair was cut short. One of my nurses and a bath-tub were brought to the house at once; it was late in the evening, and when I saw my patient next morning a very satisfactory change had taken place. Her muscles were relaxed; her arm was resting; she protruded her tongue when asked to do so. After twenty-four hours of such treatment she had gained six hours of sleep, and thus improvement went on. In a few days she was sleeping at most all the time, something not uncommon with the Brand method. As the brain symptoms vanished we used the affusions more sparingly and decreased the temperature of the bath, but never used baths more than ten minutes. In due course of time the patient made a good recovery and never gave the nurse a particle of trouble in the application of the baths.

The change brought about by the treatment was so striking that I could understand how persons can become enthusiasts on the water treatment.

It may be objected that there was perhaps just then a natural change for the better in the patient's condition, but the observation of the case showed that this case was not so. When taking charge of the case we still had before us, as the temperature chart demonstrated, an acme, or high fever of from ten to twelve days, during which time diarrhœa also set in, so that we had to give 115 baths. To refute some objections I desire to add:

1. The just objection to the method, that it is so unpleasant to the patient, is at times placed strongly in the foreground. Now let us take this case: Let us place the agony of heart which the mother and father, the grown sister and others had to undergo seeing their daughter or sister hour after hour in the condition described, on one side of the scale; let us add the feelings of the patient—though delirious, such patients are not without suffering—and the shivering and chilly sensations of the patient—lasting only minutes at a time—on the other side, and then let us ask which side represents the most suffering? It must not be forgotten that the patient for her suffering lasting a few minutes earned hours of sweet natural sleep, which causes the time to pass rapidly, while a day appears as long as a week to a patient not using the baths. Time and again have I seen that with the appearance of the tub and the nurse quiet, cheerfulness, hope and systematic work have taken the place of worry, despondency, turmoil, and ignorance of what to do in the whole household. My female patients, particularly, have given me little trouble, and I have just finished a case where a young lady required 150 baths. She was anxious that the method should be carried out perfectly, and gave her mother no trouble. I cannot see why, in well-regulated households, especially among the more intelligent class. this method cannot be carried out.

2. Then there is another very important matter to which I should

like particularly to call the attention of those who are using the baths. We all know that the most perfect results with the method have been obtained in the military hospitals. That this does not depend on the mild character of the disease among soldiers can be shown by the observation that the mortality from typhoid fever normally is high among soldiers, e. g., having been from 30 to 35 per cent. during three successive years in the French army, and from 20 to 26 per cent. in the German hospitals where, under the hydriatic treatment, it has sunk to from 2 to 5 per cent.

We have to look for an explanation somewhere else. The soldier is treated from the very beginning of his illness, while in private practice the patient is often not seen by his physician until he has been ill for from seven to ten days. Time and again I have been called to cases where the patient has been not only ill for a week, but even engaged in some work. He would not have been able to do it but for a characteristic of this malady, which I have not seen discussed, but which is of especial interest to those using the baths, and to which I wish to call especial attention.

It seems that the typhoid fever poison so influences the brain that it renders the patient indifferent to his condition, depriving him of a desire to call a physician, which makes him procrastinate, waiting from day to day for improvement. It is almost the rule that a member of the family insisted on a physician being called and not the patient. The patient would have waited still longer if he would have been allowed. I have seen that persons, who are otherwise careful about their health, act in this manner when typhoid fever attacks them.

Now, if we want to see first-class results from the baths we must get our families interested in this matter. They must be informed that the initial symptoms often are very mild and meaningless, that mild initial symptoms do not indicate a mild case; we must call their attention to this peculiarity of the disease, so that they will do for the patient what he will not do for himself—call in a physician.

Only if we can have in private practice what military discipline does for the army physician, i. e., if the patient is placed under our care from the very beginning of his illness will our results be as good as in the military hospitals at Pomerania and Munich.

To obtain these results we must, of course, have the assistance of the laity; they must be interested in this matter; learn of the efficiency of the Brand method, and of the importance of an early use of the baths.

How useful a clinical thermometer in a family may be in detecting a typhoid fever case early I need not mention.

CYSTOSCOPY AND URETEROSCOPY WITH EXHIBIT OF INSTRUMENTS AND REPORT OF CASES.

By J. W. LONG, M.D., Richmond, Va.,
Professor of Diseases of Women, Medical College of Virginia.

Only recently has the bladder received the attention it deserves from gynecologists. Up to the last few years it was difficult to explore that viscus to determine the condition of its inner surface, and more difficult to explore the ureters. Prior to the invention of the instruments I am about to exhibit we were obliged to depend upon the dilatation of the urethra by a very crude method; indeed, stretching by the fingers, which often brought on permanent incontinence, is within the recollection of most of us. To explore the bladder was almost impossible because of collapse of its walls, and, as said before, exploration and treatment of the ureters was attended with even more difficulty. For many years distension of the bladder with water and fishing around for the mouths of the ureters were our only resources. Not until Kelly accidentally struck upon the method to be described could we satisfactorily treat cystic and ureteral troubles. The method depends on first, distension of the bladder with air, which, a few years ago, was thought to be exceedingly dangerous; and second, instruments whereby we may have a direct view of the inside of the bladder and the mouths of the ureters.

The instruments I show you are first, sixteen dilators, graded according to their measurement by the metric system. Numbers 10 to 14 are those usually employed. It is surprising to note the amount of stretching the female urethra can stand.

The second instrument is the calibrator, so called because originally it was intended to measure the size of the urethra. It is cone-shaped and graduated, and is now used in place of the original dilators.

At an early stage in the work it was found that the external meatus was the point of greatest resistance in dilating the urethra.

The third instrument is the cystoscope (made in various sizes), consisting of a straight tube flanged at one extremity and having projecting at an angle from near the same extremity a handle. Fitting in this is an obturator. After the urethra is dilated the cystoscope is forced in by a gentle rotatory movement, and when the obturator is withdrawn air rushes in, distending the bladder. I have suggested to Dr. Kelly that the cystoscope might, to advantage, be made shorter and with a more flaring flange.

The position the patient was made to assume at first was the dorsal, with hips much elevated I have devised a pelvis elevator for this purpose. The bladder is first catheterized, the urethra dilated, the cystoscope introduced, and the residual urine drawn off by a suction apparatus, but it is almost a matter of impossibility to keep the bladder entirely clear of urine. A better posture is the knee-chest, because in it the residual urine gravitates from the floor of the bladder to the most dependent part, and the suction apparatus may be dispensed with. After exploring the entire inner surface of the bladder, which can be seen perfectly, it is easy to find the mouths of the ureters. Any kind of clear, reflected light can be used with a head mirror.

Through the cystoscope can be introduced the ureteral searcher, which is a small sound with the handle bent at an obtuse angle. At first this is a difficult procedure, but practice makes it easy. I tried for eighteen months to catheterize the ureters, and had about decided that it was one of the things I could not do when I suddenly hit upon it, and can now introduce the ureteral catheter in a few seconds.

The original ureteral catheter was devised by Simon. Pawlik modified it, and Kelly perfected the Pawlik instrument.

With a delicate pair of mouse-tooth forceps it is not at all difficult to pick a piece of cotton, stone, etc., from the bladder; or, with an applicator, to make an application to its walls.

Whale-bone bougies are employed, first, to determine whether or not the ureters are patulous; second, to protect them during hysterectomy.

The vesical balloon, devised by Dr. Clark, consists of a fine rubber bag with a stem of rubber tubing. It is used for the treatment of chronic vesical catarrh where there are marked changes in the bladder walls, and which are not amenable to treatment by ordinary applications. He employs it covered with a 10 per cent. gelatole of ichthyol, melting at the temperature of the body. The balloon is buttered with the gelatole, rolled up, grasped in a pair of special forceps, introduced through the cystoscope, and distended with air, the medicine thereby reaching every part of the inner surface of the bladder. To prevent injury or rupture of the bladder it is important to learn, first, how far the balloon should be distended.

By means of the cystoscope it has been discovered that many of the so-called cases of cystitis, especially in young women, are not general inflammations of the vesical mucosa, but hyperæmia, or, at most, limited inflammations; and many of them are easily and rapidly cured by mild solutions of nitrate of silver or any other astringent, applied through the cystoscope. I shall cite only one or two cases to demonstrate its efficiency.

Case I. was that of a young woman (married), who, for years had cystic trouble. Under cocaine, anæsthesia, the cystoscope being introduced and the area of inflammation determined, three or four applications of nitrate of silver sufficed to cure.

Case II. a young woman whose tubes and ovaries had been removed two years before I saw her for probably ovaro-salpingitis. After the operation ~~neutral~~ ventral hernia occurred and she came for treatment. Inquiry revealed more suffering from the bladder than from the hernia and brought to light the fact that for thirteen years she had been the victim of cystic trouble. There was very little pus in the urine, but a slow, irritating, aggravating cystitis was present. Applications of nitrate of silver rendered it worse. Then Dr. Clark's method was employed. This, at first, was painful. Four applications at intervals of three days were made, and now there is complete cure. I afterward relieved the hernia, which was as large as a peck measure, by operation.

I wish it clearly understood that I claim nothing original in this work. Dr. Kelly should be credited with a large part of the recent advances. Knowing that I was working along the same lines, he has urged me to report my results, which heretofore I have not done, and which I hope to do more extensively at no distant day.

SELECTIONS.

THE SOLVENT PROPERTIES OF THE BUFFALO LITHIA WATERS OF VIRGINIA.

By GEORGE HALSTED BOYLAND, M.A., M.D. (Paris), Etc.
Formerly Professor in the Baltimore Medical College, Etc.

It has been said that "if we wish to ameliorate the human race, it is in medicine that we must seek the means."

Time has amply demonstrated the truth of these words, and in no department of therapeutics have so great services been rendered as in the treatment of disease by mineral waters. Experience has permitted us to add as a corollary to this, that if we wish to dissolve concretions from the blood and different viscera and prevent their reformation in the sanguinous fluid as well as their deposit in the organs of the body, it is the Buffalo lithia waters of Virginia that we must prescribe.

The Great Physician has provided for the sons and daughters of men a remedy so subtle in its composition and so sure in its working as to excite the wonder of the most profound chemist and the most skilful practitioner. Let these waters be analyzed and the component parts be used in precisely the same proportions, as regards weight and chemical purity, in compounding an artificial water, presenting an exact reproduction of the natural water taken from the spring, and, by some mysterious process of Nature that we know not of, their solvent power is diminished or reduced to zero. Still more inexplicable is the fact that the salts composing the Buffalo lithia waters of Virginia act with considerable rapidity in dissolving uric acid and other calculi, whether heptic, renal, or vesical, in very weak solutions, while stronger ones are less active or fail altogether. Dr. Roberts, of London, has given us very valuable data on this subject (vide Archives of Medicine, vol. iii). He placed fragments of uric-acid calculi, weighing from forty to a hundred and twelve grains each, in ten ounce phials of carbonate solutions, analogous to that found in Spring No. 2 of the Buffalo lithia waters, these solutions being of various strengths. The experiments were continued day and night for some time, the daily flow of water varying between six and fifteen pints. Operating in this way, he found that above a strength of a hundred and twenty grains to the pint no solvent action was exerted, and even with eighty grains to the pint there was only a little; but in solutions of sixty grains to the pint the solvent action was more noticeable, but began to act decidedly at fifty grains to the pint; at forty grains to the pint the activity was further increased, and so on down, the solvent action being stronger and more speedy as the strength of the solution was diminished and approached the quantities contained in the analysis of Spring No. 2, which is justly considered to be the most powerful and perfect solvent of the various forms of calculi known to science, and not of calculi only, whether of the red lithic or uric acid or the triple phosphate of ammonium-magnesium variety, but also of morbid concretions in general, such as albumin, sugar, cholesterin, crystals, etc.

Many years ago, while I was resident physician at these world-renowned springs, a patient suffering from renal calculi of red uric-acid composition, wishing to assure himself of the potency of the Buffalo lithia waters as a solvent, placed a small bit of calculus passed by himself, and of about the size of the head of a large pin, in a goblet filled with the water of Spring No. 2, leaving the same covered on a shelf in his bed-room. Two weeks later he brought us the intelligence that the bit of calculus had entirely disappeared in solution with the water. Wishing in turn to verify the accuracy of a statement of such scientific importance and value, I at once undertook the following experiments: Ten grains of a red lithic-acid calculus and an equal quantity of a triple phosphatic deposit of the whitish ammonium-magnesium formation were placed side by side in separate glass receptacles of Buffalo lithia water, Spring No. 2, containing each thirty ounces, and left standing. These vessels were not moved or touched in the slightest manner throughout the whole duration of the experiment, but remained covered with glass slabs high out of reach, for it was desired to test the solvent power exercised by the chemical action of the water upon these two typical varieties of calculi.

At the end of two weeks disintegration had already begun and at the end of the third week the calculus had separated into three smaller fragments, which in another eight days presented a clean, water-worn appearance, with a few loose flakes floating on the surface of the water. At the end of six weeks all traces of the stone had disappeared, solution being complete. This was with the red uric or lithic-acid formation. With the other, the whitish-gray triple phosphate of ammonium-magnesium deposit, the process was somewhat different, but accomplished in the same lapse of time. Here, a whitish coating seemed to form around the original calculus, which softened it, causing it to break into many pieces resembling very coarse sand, and finally to dissipate itself and vanish altogether. The same experiment was then made with flowing water; that is, the water of Spring No. 2 was so arranged by means of a spigot and retort as to be continually renewed and to constantly wash the same amount of pebbles and of the same two varieties in the bottom of the retorts. It was found that exactly the same solvent process took place, but instead of six weeks, thirty days had sufficed to cause all traces of either stone to disappear. Analogous experiments were then undertaken with the waters of Springs Nos. 1 and 3; with the result that the water of Spring No. 1 acted a little more promptly in dissolving calculi of the triple phosphate of ammonium-magnesium than those of uric-acid concretion. Nevertheless, at the end of the thirty days dissolution was complete in both, the experiments having been carried out in every detail like those made with the water of Spring No. 2.

As regards those made with the water of Spring No. 3, the results obtained in the same manner and at the end of the same period of time, other things being equal, were as follows: Neither of the calculi was dissolved. Both, however, were carefully examined after the time prescribed by the first experiments had elapsed, and both were found to be coated with a whitish layer of about the consistency of pulp; then a second layer of the stones had become softer, the interior being slightly more friable. Neither was there any breaking into fragments, as was the case with the waters of Springs Nos. 1 and 2, and the condition of the two specimens was much the same. From this it was concluded that while the waters of Springs Nos. 1 and 2 possessed a solvent power superior to any other known mineral waters, that of Spring No. 3 had also a solvent potency, but in a comparatively feeble degree, which furnishes a valuable therapeutic indication that will be referred to later on.

But let us come to the solvent action of the Buffalo lithia waters of Virginia inside the human body.

A chemical analysis of the waters, made by Professor William P.

Tonry, of the Maryland Institute, Baltimore, gives the following results (expressed in grains to the imperial gallon):

	Spring No. 1.	Spring No. 2.	Spring No. 3.
Sulphate of magnesium	1.530	[illegible]	0.150
" aluminum	8.180	9.017	3.035
" potassium	0.463	......	......
" calcium	19.251	33.067	2.353
Carbonate of potassium	....	29.300	1.[illegible]52
Bicarbonate of calcium	39.277	14.963	2.524
" lithium	1.484	2.250	traces.
" iron	0.500	0.300	3.774
" barium	....	1.750	......
Chloride of sodium	1.256	4.921	0.21
" silicon	1.725	1.873	0.57
Phosphoric acid	traces.	traces.	traces.
Iodin	traces.	traces.	......
Organic matter	small am't.	small am't	small am't.
Total grains to the gallon	73.666	98.376	14.47
Sulphureted hydrogen (cubic in.)	5.9	8.3	3.4
Carbonic-acid gas (cubic in.)	09.1	59.2	11.6

It will be observed from the analyses here appended that Spring No. 2 contains 29,300 grains of carbonate of potassium, 2,250 grains of the bicarbonate of lithium, and 4,921 grains of the chloride of sodium, a mixture made in such exact proportions when combined with the other ingredients as to surpass the most ingenious formulæ of materia medica.

Our friend and confrère, the late Dr. J. Milner Fothergill, of London, whose authority in such matters is recognized all over the world, said: "In all cases of affections of the liver and kidneys, but more especially where uric acid and lithæmia are the prominent symptoms, "soda and lithia for the liver, lithia and potash for the kidneys," (vide Fothergill's Handbook of Treatment) are the sovereign remedies.

My own experience covers at this late day many thousand cases in which we have prescribed and observed the solvent effects of the Buffalo lithia waters, not only at the springs themselves, but in different parts of the United States and on the Continent of Europe, and I can only repeat what I have already said regarding their action when taken internally: that in the class of cases in which lithia, soda, and potash are regarded as most specially indicated I have obtained far better results from the Buffalo lithia waters than from any of the preparations of the lithium salts of the pharmacopœia; furthermore, I am satisfied that there is no other mineral water either in America or in Europe so singularly adapted to such a large number and variety of maladies.

The solvent properties of all three springs on grape sugar is immediate (as can be readily proved by placing ten or twenty grains in a test tube and adding half an ounce of water), and their great value in the treatment of diabetes mellitus is attested by numerous cases. Although the solvent properties of these waters may be less evident in vitro as regards albumin, that exercised upon the albumin in corpore and in the blood in albuminuria and icteric fever, which it dissolves as it does the biliary elements and cholesterin crystals contained therein in the latter disease, it as remarkable as it is in lithæmia, lithiasis, and the different forms of hepatic, renal, and vesical calculi. Albumin, biliary constituents, such as cholesterin crystals and gallstones, sugar, uric acid, and phosphatic calculi are characteristic of so great a variety of diseases that they may be said to dominate all pathology; and it is because of the active solvent potency upon these different morbid concretions that the Buffalo lithia waters of Virginia are unrivaled in the treatment of so wide a range of pathological process.

It will be impossible to do more than narrate briefly one or two cases at most of each variety, from among the thousands treated by ourselves and others by these waters; the limits of a single article forbidding the introduction of long lists of cases in detail. Cases Nos. I and II are selected as typical of their class of calculi:

Case I.—Mr. A. came to the springs with the following symptoms: constant pain in the lumbar region, being obliged to walk with his right arm across his loins as a support, of florid sanguineous temperament, nodosities of Heberden on the fingers, tongue heavily furred, passing quantities of red uric-acid gravel varying in size from the head of a pin to that of a large pea, and having had frequent attacks of nephritic colic. After a six weeks' course of the water of Spring No. 2, he returned home cured.

Case II.—Mr. G.'s was a case of the triple phosphate of ammonium-magnesium variety. This patient was small in stature, emaciated, and pale, passing almost continuously and to a considerable amount calculi, accompanied by pain in vesical region and tenesmus of the sphincter, the urine being mixed with blood and mucus. He began upon three glasses a day of Spring No. 1, which were increased gradually to six and then to eight. At the end of ten days a decided improvement had taken place; the pain was much alleviated, the blood and mucus having entirely disappeared from the urine. In another week the calculi had taken the consistence of sand and at the end of four weeks all symptoms of disease had vanished. It was now that he was put upon the water of Spring No. 3, three glasses a day, in order to prevent the reformation of calculi and to overcome the anæmic condition.

Case III.—Mrs. L., gallstones. This patient, like almost all others that visited the springs, had been under treatment for some time previous, having exhausted the customary remedies, such as chloroform, morphin, lithia in powders, ether in capsules, etc., without relief. The attacks had been succeeding each other every few days at the time she consulted us at the springs. Owing to some existing gastritis, the water, that of Spring

No. 2 had to be begun in very small quantities, half a glass four times a day. In forty-eight hours the gastric symptoms were already alleviated and the quantity of water could be rapidly increased. This patient only had one subsequent attack, and that shortly after her arrival at the springs, which she left at the close of the eighth week; the jaundiced color of the skin habitual with her having given place to a healthy whiteness with pink cheeks.

CASE IV. *Suppressed Gout.*—Uric acid in excess was found in the urine; there was a scarcely perceptible tophous roughness here and there along the border of the helix of the right ear, but that was all. Some emaciation. Four glasses a day of Spring No. 2, for twenty-eight days rendered his urine perfectly normal and gave him a gain of ten pounds in weight.

CASE V. *Gout.*—This was a large, plethoric woman, weighing about two hundred pounds, who left the springs after a five weeks' course of Spring No. 2 very much benefited and having lost twenty pounds, much to her satisfaction. Repeated examinations of the urine showed it to be free from uric acid, and no crystals could be obtained after the second week; the chalky deposits and tophi in the joints had been considerably reduced in size, the smaller ones having been resorbed completely.

Unfortunately, this patient was unable to finish her course, being obliged for family reasons to return home, otherwise we would have had a restitutio ad integrum. By exerting their solvent power these waters accelerate the exchanges, prompting at the same time nutrition and assimilation, thereby regulating the proportion of adipose to muscular tissue.

Now follow a number of cases of lithæmia and lithiasis, which ought really to be classed as cases of uric-acid and prosphate dyscrasiæ. Lithæmia, properly defined, is the dyscrasiæ of calculus or gravel and lithiasis the complication of calculi themselves, and the term may be practically applied to all cases of spurious or abortive gout, hepatic, renal and vesical disease characterized by mixed lithic formations not described in pathological nomenclature nor belonging to any regular pathological anatomical class. In such cases, the waters of Springs Nos. 1 and 2 act with equal promptness, purifying the blood by dissolution of the lithic elements contained therein; preventing their reformation in lithæmia and dissolving the calculi in lithiasis. In this series of cases all the calculi were of the uric-acid and triple phosphate of ammonium-magnesium varieties combined.

Stone in the bladder, whether of renal or vesical origin, and whether of oxalic, uric, or phosphatic formation, is an indication par excellence for the exhibition of the Buffalo lithia waters. Of the cases cured by them, perhaps the most striking is that of Mr. C., of North Carolina, who came to the springs with a large single stone in the bladder. This case places beyond all doubt the wonderful solvent power of these waters. Mr. C.'s stone, which was a triple phosphatic one, became first disintegrated into smaller fragments, these in turn into gravel, and that eventually into fine sand, until finally there was no discharge of calculus concretion in any form, and the urine, from having been strongly ammoniacal, had become perfectly normal. I received news of this patient one year later and there had been no return of the disease.

Jaundice, which is quite a common complication in hepatic disease of almost every form and characterized by the retention in the blood of the biliary constituents, chief among which are cholesterin crystals, is quickly relieved by these waters, which dissolve the crystals and alkalize the blood.

Albuminuria.—These cases count by hundreds There is no remedy so absolutely specific in all forms of albuminuria and Bright's disease, whether acute or chronic, as Buffalo lithia water, Spring No. 2, accompanied by a milk diet. In all cases of pregnancy were albumin is found in the urine as late as the last week before confinement, if this water and a milk diet are prescribed, the albumin disappears rapidly from the urine and the patient has a positive guarantee against puerperal convulsions.

We have treated quite a considerable number of diabetic and glycosuric cases with the waters of Springs Nos. 1. 2 and 3, and. where the patient persevered in the treatment to the end, with a perfect cure for a result; in others, with marked benefit; always relying upon Springs Nos. 1 and 2, at times singly and at others in combination, for the main treatment, and on Spring No. 3 as an adjuvant and chalybeate to overcome the anæmic condition usually attending such cases. The solvent action is here most marked, not only as regards the glycogen in the blood, but also the hippuric acid contained therein, and which Prout and Garrod first found in diabetic urine. (See Beale on Urinary Deposits.)

The two following cases of diabetes mellitus are taken from our notebook as typical of the two great classes into which all diabetic diseases are grouped—viz., diabète gras, fat diabetes, and thin or pancreatic diabetes.

Mr. B. was addressed to me by the late Dr. Van Bibber, of Baltimore, and when first seen by me at the springs his urine contained large quantities of sugar, three drachms to the quart, with a density of 1.045. He had marked polyphagia, polydipsia, and polyuria, being a large, fat man of the whitish type, the increase in weight coinciding with the development of the diabetic symptoms. He remained ten weeks at the springs, the quantity of sugar diminishing from the very beginning of the treatment, the appetite and abnormal thirst growing less pari passu until at the end of the eighth week all diabetic symptoms had entirely disappeared, he being completely restored to health; it was nevertheless considered advisable to continue the treatment two weeks longer, but in diminished quantities; four glasses of water, two of Spring No. 1 and two of Spring No. 3, were drunk during the day. This patient drank during four weeks the water of Spring No. 2 and during the second four weeks that of Spring No. 1.

The other case was that of a young lady, seventeen years of age, suffering from nervous crises, pain in the lumbar region, wasting, the urine loaded with sugar, and the patient presenting all the symptoms of pancreatic diabetes. The waters of Spring No. 2 were ordered, and fourteen days after she began to take them there was a great diminution in the quantity of sugar observable; the nervous crises had ceased, also the pain in the lumbar region. To my regret, this young lady was now obliged to leave the springs and the case was lost sight of.

The number of cases of simple accidental glycosuria without diabetic symptoms cured by the Buffalo lithia waters is much more considerable; such cases are generally promptly relieved in a few days, sometimes in twenty-four hours.

Appendicitis.—Where appendicitis is dependent upon the formation of phosphatic deposit in the appendix vermiformis, the waters of Springs Nos. 1 and 2 will prevent a reformation of calculus after the operation. It is in this way that the best results will be obtained, unless it be as a prophylactic, for phosphatic appendicitis is a danger that patients with the lithic diathesis always run. During my last year as resident physician at the Buffalo lithia springs, one case of appendicitis that came under my care made a very good recovery without operation, drinking the water of Spring No. 1 throughout the entire duration of the disease.

It will be observed from the foregoing, which must be considered rather in the light of a résumé, that Springs Nos. 1 and 2 possess to an equal degree the solvent power which makes these waters superior to all others, not excepting the famous waters of Carlsbad. I should, however, give the preference to Spring No. 2 in all cases of the red lithic or uric-acid dyscrasia, and to Spring No. 1 in the triple phosphate of ammonia-magnesia variety, although perhaps as many cases of the latter have been as successfully treated with the water of Spring No. 2, which is at the present day the more known to fame. I cannot, in closing, insist too strongly upon the value of Spring No. 3 as an adjuvant in cases where anæmia is a symptom, not only in lithic cases but in albuminuria, diabetes, and general pathology as well. A peculiarity of the first of the two diabetic cases mentioned above was a certain proportion of inosite or muscular sugar contained in the urine. Professor Bouchard and Professor Potain, of Paris, have recently reported each a similar case.

Springs Nos. 1 and 2 hold in solution the same mineral constituents, and it is doubtless the change in their composite quantities that makes the difference in indication for their exhibition. These waters we have found equally active and efficacious in our hospital and private practice remote from the springs; the only advantages derived from drinking them in loco are those of charge of air, scenery, and cooking, which, although valuable adjuvants, are by no means essential. Of course, it must be understood that a proper diet was prescribed, and must be in every individual case while using the waters; this certainly aids their curative action, and although they are not a panacea they are at least a specific in a very large category of diseases.

Finally, were I called upon to state what mineral waters were applicable to the greatest variety of cases in which lithia, potash, and soda are especially indicated, and what mineral waters prescribed by themselves alone, leaving medicines and diet entirely out of the question, will effect the most speedy, radical, and permanent cure in such, I should unhesitatingly answer, "The Buffalo lithia waters of Virginia." I consider the salts of lithium, as held in natural solution in these waters, far more efficacious than any pharmaceutical preparations of those salts.—N. Y. Medical Journal.

TREATMENT OF COLD ABSCESSES.[1]

By HOMER GAGE, M.D., Worcester, Mass.

In considering thus briefly the treatment of cold abscesses, I have limited myself to those which occur in connection with tubercular disease of the bones and joints, omitting all reference to the glandular and other varieties. What to do with these abscesses has long been the subject of much controversy—on the one side the true orthopœdist, with his cautious conservatism, urging that they both be left largely to nature, and on the other the general surgeon, with his stern adherence to the old dictum that pus must be removed whenever found, urging the policy of active interference. There has been much discussion, but there is a very noticeable absence of any definite statistical information, especially from those who believe in leaving these abscesses to nature; and the controversy seems likely to continue as an expression of individual experience, opinion and prejudice.

One thing has, however, been made plain to us all and should be recognized as settled beyond dispute, namely, that the abscess is always secondary in importance, as well as in development, to the primary lesion, whether that is situated in the bone or in the joint. And it follows from this, that whatever methods are to be adopted in dealing with these secondary complications, they must be such as will interfere as little as possible with the treatment of the primary disease. This is, I think, the fundamental principle underlying this whole discussion, from whichever side it is viewed. The presence of pus diminishes somewhat the chance of an ideal result. There is a little more liability to ultimate deformity and limitation of motion; but this is very much less than was formerly supposed, and will be still less in proportion as the abscess is prevented from interfering with the continuance of effective mechanical treatment.

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[1] Read before the Massachusetts Medical Society, June 9, 1896, and recommended for publication by the Society.

The possibility of a spontaneous disappearance of a cold abscess by inspissation and absorption is now nowhere disputed. Many cases have been reported by Judson, Ridlon, Schaffer, Gibney and others. I have myself seen one psoas abscess and one abscess in connection with tubercular disease of the hip entirely and permanently disappear. Such instances are, however, relatively very few—far too few to be held out as an inducement never to interfere, unless it can be shown that by letting them alone we in nowise increase the difficulties or dangers of their subsequent management, in case they are not absorbed.

There are, moreover, many cases in which pain, loss or appetite, anæmia, or other constitutional disturbance seem to be dependent upon the presence of an abscess, where interference seems imperative, as it does also where the location of an abscess would prevent the use of suitable apparatus. Such cases cease at once to be the subject of controversy. The debatable ground is that occupied by those whose development is unaccompanied by any constitutional disturbance or by any interference with the continued use of mechanical support. Are they to be opened as soon as recognized, or should they be allowed to remain until they are ready to discharge spontaneously?

When pus has escaped into the inter-muscular spaces about the bone or joint in which it has originated, the abscess tends to increase slowly, but steadily, in the direction favored by gravity. Even after fluctuation has become quite superficial it may continue to spread for weeks or even months before spontaneous rupture takes place. It is liable all the while to secondary infection through the skin by more active pus-producing bacteria; its spontaneous opening is more likely to be so situated as to provide insufficient drainage, and the larger the abscess cavity, all of which is lined with a tuberculous membrane, the longer time will be required to heal it, with at least an equal chance of general or systemic infection. These reasons are, I think, sufficient to prevent us from allowing even those abscesses which do not give rise to any constitutional disturbance or interfere with mechanical treatment, always to pursue their natural course. If, however, it is admitted, as I think it should be, that the chance of septic infection of the sinus or abscess cavity is as great after a natural as after an artificial opening, and that the length of time during which the discharge will continue depends upon the condition of the bone or joint from whence it comes rather than upon the manner in which it has escaped through the skin; if this be admitted, and I do not see how it can be denied, there is no advantage to be gained from leaving any of these abscesses alone, except those which may spontaneously disappear. Can we distinguish these beforehand?

In two cases of abscess complicating tubercular disease of the hip recently operated upon by me, cultures and cover-slip preparations were taken from each. In the first, the culture tubes were sterile, while the cover slips showed the presence of tubercle bacilli; in the second, the culture tubes revealed the staphylococcus albus and citreus, while the cover slips were negative. It is, I think, unusual, even in undoubted tubercular cases, to be able to demonstrate the presence of the bacilli; but even when they cannot be found and the cultures are sterile, inoculation experiments prove the tubercular character of the pus. There are, then, as typified in these two cases, two distinct sorts of pus occurring in connection with bone and joint tuberculosis, namely: That which arises from the tubercle bacillus alone, and that which is due to a mixed infection from the simultaneous presence of the tubercle bacillus and other pyogenic cocci. When a mixed infection is present, even if there are no evidences of constitutional impairment, it is exceedingly unlikely that spontaneous resolution will ever occur. Those abscesses which disappear after one or more aspirations are, as a rule, instances of pure tubercular infection, and it is probable that such is also the character of those which disappear of themselves. If, then, we could distinguish beforehand the cases of pure tubercular infection from the mixed infections, we should have reasonable grounds for leaving the first to nature, so long as their growth was not excessive and until they threatened to open spontaneously, and for operating always on the second. Can we make this distinction? In this connection I am under great obligation to Dr. Richard C. Cabot for permission to refer to an unpublished article by Dr. John Dane, bearing upon the relation of leucocytosis to tubercular inflammations. Dr. Dane has shown bacteriologically, and the work is confirmed by Dr. Cabot clinically, that "in pure tubercular abscess, as in all purely tubercular lesions, leucocytosis is absent. If at any time the infection becomes a mixed one through the advent of pus cocci, leucocytosis occurs. For instance, many cold abscesses of rib, hip, vertebræ, etc., show no leucocytosis till opened, then the white cells rise." If these observations shall be confirmed by further investigation, I think it fair to assume that active interference should be postponed so long as leucocytosis is absent. For the same reason, those cases in which cultures obtained by aspiration remain sterile, may profitably be left to themselves. The presence of leucocytosis or of colonies of pus cocci in our cultures is an indication for active interference. When should the opening be made?

We are speaking now not of the cases in which the patient's general condition or the proper management of his bone or joint lesion are affected by the presence of the abscess. These should be operated upon as soon as such interference is recognized. Where there is no such interference, I think the opening may be advantageously delayed until the abscess has become quite superficial; such a course will allow the opening to be made more easily, accurately, and with less dissection through healthy or uninfected tissues. I should favor interference rather than waiting for spontaneous opening, because I can control the point of drainage, prevent the unnecessary extension of the tubercular process, and the better control the chance of infection from without.

What of the methods of interference? Many have been suggested, of which the most important are: Aspiration; aspiration with irrigation, and iodoform injections; simple incision; and free incision, with curettage. Aspiration has never seemed to me a scientific procedure, except when undertaken merely for exploratory purposes, and in my hands has simply resulted in an ultimate sinus at the point of the insertion of the needle. Aspiration, with irrigation and the injection or iodoform, was a few years since heralded as a specific for these local tubercular lesions. Most encouraging reports came to us from Billroth, Mikulicz, Bruns, and von Moorhof. The method has, however, never yielded such brilliant results in this country, and, although enthusiastically championed by Senn, has never found favor in our true orthopœdic clinics. The real issue lies, I think, between simple incision and free incision with curettage. The latter aims to remove all tubercular product, and secure, as near as possible, primary union. Its advocates seem to overlook the source from which the pus started, and from which it will continue to come, until it is itself exhausted. In too many of these cases the primary union is gained but for a short time, only until the sinus leading directly to the original disease can be re-established. It is forgotten that the abscess is but a secondary consideration—the original disease of the bone or joint the primary and all-important one; and that the length of time during which suppuration will continue depends almost entirely upon the condition of the original lesion, and has comparatively little to do with the method by which the abscess is treated.

It is not at all uncommon, when an abscess opens spontaneously at its most dependent part before it has become very large, and when it has not been in any way interfered with, for us to see early, spontaneous healing and gratifying results, so far as the restoration of function is concerned.

While I believe that the situation of the drain and the limitation ot the growth of the abscess make the artificial opening the better one, I think we can learn much from nature's own method. I was very much impressed, when the house officer at the Boston Children's Hospital, with a series of cases treated by Dr. A. T. Cabot with a small opening made in the most favorable place and without any interference with abscess cavity by irrigation or curette. I have since occasionally practised the same method myself, and I believe that it approaches more nearly the natural one, causes less constitutional disturbance, and is less likely to result in constitutional infection, than other more radical measures; and when undertaken with strict precautions to prevent infection from without, seems to me to be the most desirable method of treatment.

RÉSUMÉ.

(1) An abscess occurring in connection with tubercular disease of the bones or joints is always secondary in importance, as well as in development, to the primary disease. Its treatment must not, therefore, in any way interfere with the treatment of the original lesion.

(2) When the abscess is accompanied by any evidences of constitutional impairment, or interferes in any way by its location with the use of proper mechanical treatment, it should be immediately opened. When there is no interference with general health or with mechanical treatment, the abscess, if it presents a pure tubercular infection, may be left until it is nearly ready to open spontaneously. If it presents a mixed infection, it is to be opened at once.

(3) All cases are to be opened as soon as they approach the surface, to avoid unnecessarily extensive burrowing.

(4) Of the methods commonly used in opening these abscesses, aspiration with irrigation, free incision with curetting, all seem to give inferior results to those obtained by simple incision in most dependent portion, with the least possible interference with the walls of the abscess.—Boston Medical and Surgical Journal.

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PROCEEDINGS OF SOCIETIES.

RICHMOND ACADEMY OF MEDICINE AND SURGERY.

Regular meeting, held at Y. M. C. A. Hall.

Dr. Landon B. Edwards, President, in the chair. Dr. Mark W. Peyser, Secretary and Reporter.

Dr. J. W. Long read a paper on "Cystoscopy and Ureteroscopy, with Exhibit of Instruments and Report of Cases." [See page 24.]

DISCUSSION.

Dr. Jacob Michaux asked Dr. Long if he had found hyperæsthesia of the external parts in the case last described.

Dr. Long: Yes, and it has been dismissed four or five months.

Dr. Michaux, continuing, said that, in these cases, he had not had uniformly good results. Those in which he had succeeded best were those in which he had cauterized liberally, according to the old methods. In conditions of hyperæsthesia of the nymphæ and meatus, he had been much discouraged. He thought, therefore, that Dr. Long's remarks were interesting and valuable, and that the new method described had opened up a new field.

He described the following case as illustrative of failure: Woman, age 36 years, primipara. There was intense hyperæsthesia of the mouth of the urethra; the desire to urinate was constant. The urethra being dilated, it was found to be inflamed, and was cauterized and washed out; the condition was thought to have been cured. The patient, who lived away from town, went home and soon reported herself as in the same condition. She returned and some little growths, caruncles, were removed from the meatus, producing no apparent benefit. Thinking, then, the trouble was due to the state of the nerves, a section of the meatus was cut out; but neither did this result in relief, nor did a second, deeper operation.

During all this period, extending over several years, suffering was intense, all treatment failing. Dr. Michaux thought it a case of cystitis, with a peculiar condition of the nerve fibers. It was one of the cases troubling him, and he would like to hear some discussion of it, with a view of eliciting instruction.

Dr. Hugh M. Taylor said that one feature should be added to Dr. Long's paper, and that was that the first suggestion as to the safety of the ureters during laparotomy was made by a Virginian, Dr. Alexander Irvin, of Evington, Va. He suggested catheterization of the ureters.

Dr. Taylor said he had not yet had an opportunity to test the efficacy of the instruments described, but had no doubt that, by their means, treatment and cure would be greatly facilitated.

Dr. J. N. Upshur remarked that, in the local treatment of any disease, we should always have one caution. The subject presented by Dr. Long was very interesting, but many diseases of organs treated locally are not primarily due to the organ itself. This is especially true of the bladder. Cystitis is often reflex, as from abrasion of the os uteri. In one case, he saw no indication pointing to disease of the bladder itself. When the treatment was withdrawn from the organ, it was cured. Retro-displacements, anteflexions, anteversions, hæmorrhoids, anal fissures, nervous conditions, all give rise to irritation of the bladder, treatment, then, directed to that organ failing to relieve.

A case in point was as follows: Woman, with an intensely irritable bladder and frequent desire to void urine without satisfaction. As a matter of curiosity, Dr. Upshur asked the patient to keep an account of the number of times she was up during one night, and was told that, between bed-time and 7:30 A. M., it was seventy times.

The woman was supposed to have been addicted to masturbation and accused of it by former physicians, but vehemently denied it, and he was inclined to believe her. There was intense hyperæsthesia of the external genitals, and specular examination revealed a stenosed cervix, with marked anteflexion. Being before the days of curettage, he dilated without this procedure; but there was no benefit except a relief of dysmenorrhœa. He could find no other trouble except a tender spot in the lumbar spine, and advised her to seek another physician, which she did, but again received no benefit. Despairing, she gave up all treatment, and in the course of time her trouble disappeared. The doctor said he did not believe any bladder conditions existed, because none of them would have got well spontaneously. He thought it a case of hysteria based upon some nervous condition. He used this case as illustrative of the remark he made in the outset, that with the development of measures for local treatment, we must not forget causes in other directions. Various deranged conditions of the stomach may cause vesical conditions, and a fairly common cause is, women do not drink enough water.

REPORTS OF CASES.

LAPAROTOMY—RECOVERY—WHITE DISCHARGE—WHAT WAS THE DISEASE?

Dr. W. T. Oppenhimer described a case, upon which he asked for information. Railroad man was suddenly attacked by violent pain in the region of the liver, which he thought was due to gall-stone. The patient passed a bad night, and by morning the pain had radiated over the entire abdomen and was accompanied by nausea and vomiting; in consequence, the diagnosis was changed to appendicitis. Dr. I. H. White was called in consultation. The pain, in the meantime, being again confined to the liver, gall-stone was the diagnosis agreed upon. The patient grew much worse; temperature fell, pulse became rapid and feeble, and tympanitis developed. In a second consultation, Dr. White advised no operation. Dr. Geo. Ben. Johnston, on seeing the case, said it was possibly appendicitis, but he, too, considering the man's condition, thought operation unjustifiable.

Dr. Oppenhimer told the patient his chances were almost hopeless;

that an operation at least could do no more harm than hasten the end, and obtained his consent. At 12 P. M., he was removed to the Retreat for the Sick and the abdominal cavity opened. A fluid, chyliferous in appearance, flowed out. Everything, so far as ascertainable, appearing normal, the gut not sloughing, etc., the incision was closed and a gauze drain inserted. The following morning the swelling had abated, the temperature had fallen, and the discharge, which was profuse, became yellow. After four weeks the latter had become inspissated and could be squeezed out, at times being so firm that it had to be lifted out with forceps. The man recovered and is now at work.

In answer to various questions, Dr. Oppenhimer stated that Dr. Hoge, on examining the discharge, found it gave the reaction of bile. The gall-bladder could not be reached. There was no jaundice. The man had had slight attacks of pain before, but they passed off in an hour or two.

CINCINNATI OBSTETRICAL SOCIETY, PRESENTATION OF SPECIMEN—DERMOID CYST.

Dr. Bonnifield: On examination per vaginam I found the uterus to the left side of the pelvis and high up, which made me think the growth was interligamentous. The fluctuation was so distinct and the tumor so uniform I believed I had a plain ovarian cyst. Upon opening the abdomen I found a cyst that presented nothing abnormal in the line of incision, and evacuated a fluid of a dark coffee-ground color. I then found the growth was interligamentous and I had much trouble in dissecting the broad ligament from it. On examination of the tumor I found numerous masses of sebaceous matter which looked like the case Munde has reported. There are several pieces of bone and some eight or ten teeth, some of them incisors and some bicuspids. Also, in the lower part of the cyst there was a fresh blood clot and an organized blood clot, showing that bleeding had evidently taken place comparatively recently. So the tumor is a mixed one, dermoid in part and an ovarian cyst in part. There was an unusual amount of stitching to be done, and at the best we could do there was a large amount of raw surface on the posterior part of the uterus. A drainage tube was left in twelve hours, but there was only a little drainage, about a dram every hour. She has had no trouble and is getting along very nicely.

ABSTRACTS.

DIAGNOSIS OF CARCINOMA OF THE BREAST IN ITS EARLY STAGES.

Dr. A. Marmaduke Shield, in a clinical lecture at St. George's Hospital, goes over this subject very carefully and clearly. The question involved in the accurate diagnosis of this condition is such an important one that a careful study of the symptoms as observed in the large experience of Shield is valuable.

He states that out of seven hundred and fifty cases of breast-diseases of all kinds at St. George's Hospital, between the years 1875 and 1895, three hundred and fifty-three, or about one-half, were carcinomatous.

In making the examination he advises that the thorax be completely undressed. The examination should be made with the flat of the fingers, and not by roughly pinching up a portion of the gland. The following points are to be noted: 1. The age; the most likely age is between forty and fifty-five. 2. The character of the pain, which is neuralgic, not throbbing, and is not accompanied by heat or redness. 3. The onset is insidious. 4. The contour is rarely globular, usually it is obtuse or irregular. 5. The hardness is stony, and is one of the most striking and reliable characteristics of scirrhus. 6. The growth does not move in the breast substance, but moves with it 7. Retraction of the nipple is an important symptom, but it may be absent. 8. The dimpling of and the pig-skin-like condition of the skin are of the utmost value. 9. The presence of enlarged glands is important, but their absence is not proof that the disease is innocent. In cases of doubt the author is strongly in favor of exploratory incision. He believes that there are peculiarities of structure or of growth in certain individuals which may be sufficiently pronounced to become hereditary, and thus a predisposition to carcinoma may be found in some families. He calls attention to the fact that certain obscure conditions coming on in elderly women, such as an insidious pleurisy, hydrothorax, supposed "rheumatic" pains about the thorax or bones, or, of still greater importance, spontaneous fracture, or severe pains in the spine, terminating in paraplegia, may be due to cancer, and that in all such cases the breast should be carefully examined, as the discovery of scirrhus nodule may explain a very mysterious illness.

He insists upon a careful examination of the breast, lungs, and abdomen, especially of the liver, for secondary growths, as having important bearings on the question of operation.—International Medical Magazine.

SPINAL PARACENTESIS.

The knowledge of the fact that a free-communication exists between the subarachnoid spaces of the brain and spinal cord through the foramen of Magendie led Quincke, of Kiel, in 1891, to puncture the spinal membranes in order to withdraw a portion of the cerebro-spinal fluid in cases of hydrocephalus, as a substitute for tapping the lateral ventricles. Later, he employed the procedure in cases of meningitis of whatever form, with the view of relieving intracranial pressure. At the time of his first report, in 1891, Quincke had performed spinal paracentesis twenty-one times on ten patients. Two years later, Ziemssen reported a number of cases so treated. In some instances he withdrew as much as ninety cubic centimeters of fluid without unfavorable results. As might be expected, from the nature of the diseases that give rise to the conditions in which this measure would be applicable, Quincke's operation has not proved to be a therapeutic agent of great value. It is said, however, to have afforded considerable relief in some cases, particularly in those suffering from acute serous meningitis.

The Boston Medical and Surgical Journal, January 23 and 30, 1896, contains an extended review of this subject, and Jacoby has added a valuable contribution to this subject (New York Medical Journal, December 28, 1895, and January 4, 1896), in which he describes thirty-five cases, embracing a variety of conditions subjected to spinal puncture. He notes that in one case the headache accompanying a brain-tumor was relieved, and the pulse and general condition was improved in cases of meningitis. Goldschisder reports the case of a child suffering from symptoms similar to those of meningitis that was cured after withdrawing eighty ounces of cerebro-spinal fluid. Freyhan records the case of a man, 20 years of age, who presented the characteristic symptoms of tubercular meningitis, going on to paralysis and coma. Two punctures were made at different times. The fluid in each instance showed the presence of tubercle bacilli. Improvement began after the first puncture, convalescence being established. Fürbringer records the case of a patient with right hemiparesis, headache, dizziness, and double-choked disks apparently cured by spinal puncture. Gaibissi reports a case of tubercular meningitis in which the headache lessened, the subcultures ceased, and the pupils became more active to light.

It must be admitted in the cases that recovered and in those that died, in which no autopsy was made, that the diagnosis will remain uncertain. On the other hand, we are irresistibly led to be less dogmatic in pronouncing these conditions incurable.

Lichtheim, Fürbringer, Jacoby, and others have employed spinal paracentesis for the purpose of diagnosis, especially between the serous, tubercular, and purulent forms of meningitis. It is also a useful means of determining the degree of pressure existing in the cerebro-spinal cavities and of detecting the presence of hæmorrhage, the various forms of micro-organisms, and other pathological constituents of the fluid. The force with which the fluid flows from the needle will give an approximate idea of the amount of pressure that exists, but as Quincke considers this a very important point to determine accurately, he prefers to connect the needle with a manometer. Under the normal pressure the fluid flows from the needle drop by drop, while under abnormal tension it escapes in a stream with more or less force.

The presence of blood in the fluid withdrawn, if not due to injury of a vein in introducing the needle has led to the detection of ventricular hæmorrhage that otherwise would have been very difficult of diagnosis. Meningeal hæmorrhage may also be thus detected.

Lichtheim observed micro-organisms in the fluid withdrawn in three cases of purulent meningitis and in four cases of tubercular meningitis. Gaibissi found streptocci and Fränkel diplocci. Fürbringer's statistics show that tubercle bacilli were found in twenty-seven of thirty-seven cases of tubercular meningitis (73 per cent.), the result having been confirmed by autopsy. In Gaibissi's cases referred to above, twenty-five cubic centimeters of the cerebro spinal fluid injected into a guinea pig caused its death from tuberculosis.

Regarding the significance of other abnormal constituents of the cerebro-spinal fluid, but little is known. Normally, albumin is said to be absent. Quincke states that the presence of a comparatively large proportion of albumin indicates a recent inflammation, and Lichtheim found that the fluid contained has albumin in cases of brain tumor than in those of abscess of the brain and of meningitis. More than 1 per cent. of albumin would point to inflammation rather than tumor. Lichtheim found sugar in all cases of brain-tumor examined, but only rarely in tubercular meningitis. It is said, however, that the normal fluid contains a substance that reduces Fehling's solution.

The needle employed for spinal paracentesis should be at least eight centimeters in length, and should have a caliber large enough to permit of the ready exit of the fluid if it be purulent. The cord terminates at about the first lumbar vertebra, below which are branches of the cauda equina. The aspiration is made between the third and fourth, or fourth and fifth lumbar vertebræ, about five millimeters from the median line, to avoid the resisting interspinous ligament. The patient should be in the sitting posture; with the trunk inclined forward in order to obtain as much separation of the vertebræ as possible. The most rigid antiseptic precautions having been carried out, the needle is gradually introduced until the cerebro-spinal fluid begins to flow, the distance being from two to eight centimeters, according to the age and build of the patient. The fluid should be allowed to flow from its own pressure and not by aspiration, as by the latter, agonizing headache has been produced. The quantity to be withdrawn depends upon the condition present and the object to be secured. Caillé gives the following directions for tapping the spinal canal: After locating the third and fourth lumbar vertebræ, place the middle finger of the left hand on the spinous process of the third and the index-finger on the spinous process of the fourth lumbar vertebræ, pressing firmly upon the bone. Next, mark with the nail of the right index-finger midway between the two fixed points, and puncture directly in the middle line with a large-sized hypodermatic needle, attached to its syringe, which is to be used as a handle only, and not for the purpose of aspirating. No undue force should be employed, as the needle readily enters the spinal canal when the syringe is to be unscrewed and the fluid allowed to flow into a sterile bottle. After performing this operation, Ziemssen has introduced a weak solution of iodine, but the result is not stated.

The puncture itself is without danger if performed antiseptically, but the possibility of spreading a cerebral meningitis to the spinal membranes must be borne in mind.

Dr. Jacoby summarizes the present status of lumbar puncture as follows: Cerebro-spinal fluid can be removed easily from the subarachnoid sac of the spinal cord and from the cavities of the brain.

Therapeutically, it is only of direct value as a palliative through the reduction of increased pressure; it may, perhaps, prove of more service indirectly as the first step to local treatment of the cord and brain.

Diagnostically, it possesses great clinical advantages in the detection of the various inflammatory affectations of the cerebral membranes and in the recognition of intraventricular hæmorrhage as well as of hæmorrhage within the spinal canal.—Editor Univ. Medical Magazine.

THE AORIS AND GALE LECTURES ON THE PHYSIOLOGICAL FACTORS INVOLVED IN THE CAUSATION OF DROPSY.

Factors involved in the causation of dropsy:

1. Factors causing increased transudation:
   - A. Increased intracapillary pressure:
     - *a.* Venous obstruction.
     - *b.* Vasodilatation.
     - *c.* Plethora.

- B. Increased permeability of vessel wall:
  - *a*. Local injury by mechanical irritants.
    Local injury by thermal irritants.
    Local injury by chemical irritants.
  - *b*. Malnutrition.
  - *c*. General injury by circulating poisons (?).
- C. Watery condition of blood (hydremia).

2. Factors causing diminished absorption:
   - A. By lymphatics:
     - *a*. Paralysis of limbs.
     - *b*. Obstruction of lymphatic trunks.
   - B. By veins:
     - *a*. Venous obstruction.
     - *b*. Watery condition of blood.
     - *c*. Concentrated transudations.

Dr. Starling states that probably under no circumstances can dropsy be ascribed to an abnormal change in one of these processes; for in nearly all cases we shall find that the dropsy is due to the simultaneous alteration of two or more of these factors.

Dropsy due to venous obstruction is the simplest in its pathology. When we meet with dropsy of one extremity due to obstruction of the veins draining that part, due either to pressure by growths or of thrombosis occurring in the vein itself, this dropsy is naturally ascribed to the increased lymph production consequent on the raised intracapillary pressure behind the obstruction. But upon experimentation, the causation is not quite so simple, for we find that there is more than one factor at work. If the femoral vein in a healthy man be ligated, we may have no ill effects whatever, and even if the lower end of the inferior vena cava be tied off in dogs no œdema of the legs result. Paschutin has shown that ligature of all the venous trunks of the dog's leg caused œdema of the foot and ankle. Rauvier has shown that after ligature of the inferior vena cava along with division of the sciatic nerve on one side, so as to produce dilatation of the arterioles on that side, the limb in which the sciatic nerve has been divided will become edematous. Injecting the veins of the leg with plaster-of-Paris has caused œdema, according to Cohnheim.

If a vein be obstructed in a normal animal, the first effect will be a rise of capillary pressure and increased exudation. Since the onward flow of blood is checked in the effected capillaries and veins, the increased exudation must be at the expense of an increased concentration of the blood-plasma; with this increase in the difference between the osmotic pressures of the plasma and lymph, and the absorbing force—i. e., the osmotic pressure of the plasma will rise until it is equal to the driving-out force—i. e., the capillary pressure. In a normal dog, with normal vessels, the processes tending to cause œdema will be pulled up almost as soon as they have started, and moderate venuos congestion will produce no œdema. Dr. S. states that in the normal animal, the factor which retains the appearance of œdema is the impermeability of the vessel wall, since it is this impermeability which maintains the difference in osmotic pressure between blood and lymph. If the permeability can be increased in any way the balance of processes will be lost, exudation will predominate over absorption, and œdema results.

A capillary wall is alive and is composed of cells which have a metabolism of their own and are dependent on a free supply of oxygen for their proper nutrition, along with nutrient material and a free exit for their waste products. If these cells are injured in any way the resistance of the membrane is diminished and its permeability is increased. Such an injury will follow if they be deprived for some time of a fresh supply of nutrient material and oxygen. It has been shown by Cohnheim that after long-continued anæmia of the rabbit's ear, the vessel became so permeable that the restoration of the normal circulation was followed by pronounced œdema of all the tissues. A long-continued venous obstruction, Dr. S. says, must affect the vessel-walls in the same way as anæmia, since here also the cells will be starved or asphyxiated, and hence it is that in the chronic conditions, which cause venous obstruction in man, we have the production of œdema.

Malignant growths occurring in the neighborhood of a vein are usually associated with a condition of cachexia and consequently an impoverished blood supply to the endothelium.

In summing up, we may say that œdema can never be brought about in the limbs by a moderate rise of venous pressure, so long as the capillaries retain their normal impermeability. So soon as the permeability of the vessels is increased œdema will follow. Any of the following ways may bring about the injury leading to this increase in impermeability: (1) Long-continued venous congestion (asphyxia of cells); (2) an excessive rise of intracapillary pressure breaking down the normal resistance of the cells; and (3) malnutrition due to an impoverished state of the blood.

The œdema, as a consequence of uncompensated or imperfectly compensated heart disease, is much more frequent. This form of œdema is generally considered due to a rise in venous pressure and consequent venous obstruction.

In the causation of dropsy in heart disease, alterations in the circulation must first be taken into account, as well as the alterations in the absorption and production of lymph. Taking the vascular system as a closed system of tubes having a definite capacity, the circulation coming to a standstill, the pressures at all parts of the system would become the same, making what is called the mean systemic pressure. The height of this mean pressure depends solely on the relation between the amount of contained fluid and the capacity of the system.

If circulation be established by the heart's beat, the relation between the capacity and the volume of blood remaining unchanged, no alteration can occur in the mean pressure. The only alteration is in the distribution of the pressure. On the venous side of the heart the pressure will sink below the mean systemic pressure, while on the arterial side the pressure will be raised above the mean pressure. If after the circulation has been established the heart's action be interfered with, as by damage to the valves, or be checked altogether, the pressures on each side will tend to return to the zero of the system—the mean systemic pressure. As a result there will be a fall of arterial pressure and a rise of venous pressure to this point. The turning-point of the circulation, so to speak, lies in the region of the hepatic capillaries in the abdomen, and about the level of Poupart's ligament in the femoral vein. The heart failing, there would result a rise of pressure in the vena cava and in the large veins of the neck; but a fall of pressure in the portal vein, in the peripheral veins of the legs, as well as in the arteries. Dr. S. states that it seems, therefore, that failure of the heart's action, to whatever cause it may be due, can only bring about a fall of pressure in the capillaries of the intestines and peripheral parts of the body.

There is another class of cases of dropsy in which the primary change affects the vessel-wall and not the intracapillary pressure. A change in the filtering membrane thus results, making it more permeable and allowing, under normal capillary pressures, an excessive exudation which is richer in proteids than is the normal lymph of the region in question. Cohnheim has classed all these cases, with alteration of the vessel-wall, as inflammatory œdemas.

Alteration of the vessel-wall may be the result of either the application of the injurious agent to the vessels of any given part or of the introduction of poisonous substances into the blood stream.

The author has shown that the chief action of poisonous bodies circulating in the blood was on the capillaries of the liver. The capillaries of the skin can also be affected by these bodies, as seen in the sudden onset of urticaria. Dr. S. believes that all cases of so-called nervous œdema can be explained by the circulation of some lymphagogue substance in the blood combined with local vaso-dilatation, which may often be hysterical or central in origin.

Dropsy of renal disease is classed by Cohnheim among the inflammatory dropsies. We are unable, says the writer, to trace any adequate mechanical cause, such as raised capillary pressure, for the increased transudation.

In all cases the primary cause of œdema is an increased transudation; and where we find dropsy we may say that in addition to the primary exudation there is also a derangement of some part of the absorbing mechanism.—Dr. Ernest H. Starling, in Lancet.

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THERAPEUTICS.

THE TREATMENT OF ACUTE DYSENTERY.

During the past year Testevin has treated thirty-six cases of acute dysentery among the soldiers at Grenoble. His treatment had for its objects: first, to decrease the number of movements; second, to render the intestinal tract antiseptic; third, to remedy the local anatomical lesion; fourth, to combat the tendency to collapse.

To diminish peristalsis he used hypodermic injections of morphin, 1-12 grain every hour, a large mustard plaster being applied over the abdomen at the same time to aid in relieving pain. In other instances he found it of advantage, if there was a tendency to febrile involvement, to apply an ice-bag over the lower portion of the bowel. Intestinal antisepsis was attained by the administration of calomel in the dose of eight grains a day, for two or three days, in association with camphor and opium. As soon as the bilious action of the calomel was established so that the stools showed its effects, it was replaced by doses of bismuth varying from 60 to 120 grains in the twenty-four hours, and this was fortified in its effects by the use of five to ten drop doses of laudanum. These doses of laudanum also facilitated the tolerance of full doses of salol, varying from fifteen to twenty grains in each twenty-four hours. For the local lesion he used injections of creosote made up as follows:

℞ Pure creosote, 15 minims;
Laudanum, 10 drops;
Milk or bouillon, 5 drachms.

To be added to six ounces of boiled water and given as one injection.

Preliminary to this, an injection containing fifteen grains of boric or salicylic acid is administered in order to wash out any fecal matter that may be in the rectum. The warm creosote injection is retained as long as possible. Three injections a day are usually sufficient, and after each one, if much irritability of the bowel exists, a very small suppository of belladonna and cocain may be placed in the rectum. If the case is very severe, rectal injections of nitrate-of-silver solution may be used.

For the collapse, hot-air baths or immersion baths of hot water, sinapisms to the limbs, and hot bottles about the patient, are ordered. To sustain the heart, hypodermic injections of spartein, caffein, and if necessary of ether, are given. Finally, if the condition is extremely grave, he gives injections of normal saline solution into the subcutaneous tissues; these injections are absolutely aseptic and given in the quantity of half a pint to a pint or more, two or three times in twenty-four hours.

By means of this treatment he has been able to cure patients who seemed to be in a desperate condition.—La Médecine Moderne, May 13.

ALCOHOL IN CHILDREN'S DISEASES.

The following is abstracted from a paper upon the subject by Dr. A. Seibert, of New York city, in the New York Medicinische Monatsschrift:

Nursing mothers should refrain from drinking beer if their infants have gastro-enteric disease.

Tea or coffee, without milk, is a better stimulant in the gastro-enteric diseases of children than is alcohol, which acts as an irritant poison in such cases.

Alcohol is contraindicated in the typhoid fever of children, he using it only in about 5 per cent. of his cases, and then only in collapse. It has no germicidal effect upon the contents of the digestive tube, but rather increases germ life, whilst it retards digestion. Cases fed upon soup water, coffee and tea recover the soonest; those fed upon milk without alcohol do not do so well, and those fed upon milk with alcohol improve the slowest.

In pneumonia alcohol is contraindicated except to combat collapse, its continuous administration in this affection does not prevent collapse, but rather helps induce it. He believes in plenty of fresh air in the sick chamber, despite the "time-honored seventy-two degrees F. rule."

He feeds these cases on soups, and gives water and stimulates with tea and coffee. Milk is given if it can be borne.

In scarlatina he uses alcohol only in septic infection from throat slough, and then only at night, to induce sleep and diminish the sensibility to local cleansing manœuvers, sometimes also giving an enema of antipyrine. This usually results in refreshing sleep, but when gastric disturbance has caused the withdrawal of alcohol, he has noticed no evil after effects.

Alcohol is useless in diphtheria, as it is no antidote to the Lœffler bacillus, and only adds to the myocarditis that so often causes heart failure in this disease. He has used alcohol in large doses in impending collapse for a day or two, but is not satisfied that it was the cause of recovery, and is inclined to attribute a favorable termination to energetic local measures that were begun at the same time. He had treated twenty-seven cases of diphtheria with serum, and all recovered but a child of eleven months, the only one in which alcohol had been used.

For nephritis he only uses alcohol as a diuretic, giving it in small doses, and only for one or two days. It is apt to set up acute nephritis in larger doses, or even in small doses, if continued more than a day or two. It is absolutely contraindicated in gastro-enteric disturbance.

FURUNCLE.

Dr. A. Robin administers during the formative period:

℞	Sulphur subl.	gr. iss.
	Pulv. camphor	gr. i 1-3.

M. Sig.—Three such capsules or tablets to be given daily.

After evacuation and expulsion of the core, the opening is filled with a paste composed of:

℞	Sulphur subl.	ʒ iiss.
	Pulv. camphor	ʒ iiss.
	Glycerine	q. s.—M.

—El Siglo Med.

Rectal Alimentation.—

℞	Cod-liver oil.	℥ v.
	Yolk of one egg.	
	Lime water	℥ x.

M. As a nutritive enema.

—Journal des Praticiens, March 14, 1896.

Pruritus Vulvæ.—

℞	Chloral camph.	ʒ ij.
	Bismuth subnit	ʒ ij.
	Aquæ rosæ.	℥ iv.

M. S.—Apply to the parts.

Or,

℞	Argenti nitratis.	gr. xx.
	Aquæ.	℥ ii

M. S.—Paint over the affected parts. —Bartholow.

THE STRYCHNIA CURE OF ALCOHOLISM AND OPIUM HABIT.

A certain proportion of the submerged 4,000 who pass yearly through the alcohol wards of Bellevue Hospital distinctly and seriously wish to have a treatment given them which will take away their tendency to periodical sprees, writes Dr. C. L. Dana, of New York, to the Medical News. Consequently the use of strychnia and the solanaceæ, with certain adjuvant tonics and moral influences, is employed in these cases. The drugs selected for use in this "cure" are those which the experience of ten years in the care of these cases has shown me to be most useful.

Selected patients, after having passed through an attack of acute alcoholism, and are convalescent, are allowed to remain two days and take the "cure." The wards of the hospital are not large enough to permit of a longer stay. Only persons who have reasonable intelligence and who show real evidence of sincerity are chosen.

The following solutions are used:

I. ℞ Strychnia nitrat.................gr. 1-15
Atropiæ sulph....................gr. 1-300
Aquæ distilled...................m x.
M. Sig.—Inject t. i. d. First day injection.

II. ℞ Strychnia nitrat.................gr. 1-20
Atropiæ sulph....................gr. 1-200
Aqua.............................m x.
M. Sig.—Inject t. i. d. Second day injection.

III. ℞ Tinct. cinchon. comp..............m xv.
Tinct. capsici...................m ½ to m i.
Tinct. solan. carolin............m ii
Viniferri amari..................ad dr. i.
M. Sig.—Mistur. stomachic., dr. i. t. i. d. Shake.

Order: One-half to one glass of milk (hot or peptonized), alternating with hot beef tea or broth every two hours.

IV. First and second nights if needed.

℞ Potas. bromid....................gr. xxxii.
Chloral hydrat...................gr. xvi.
Tinct. valerian..................oz. f.
Aquæ.............................ad oz. iv.
M. Sig.—Dram i dose, repeated once if needed. Shake. Mistur. sedativ.

The patients are given the injections I and II and "stomachic" III three times a day, with abundant nourishment, washing out the stomach, if necessary, to help any catarrhal disturbance.

The patient, during the treatment, is made to understand distinctly that he is taking a "cure," with all that implies, but no mystery is made of its character or of the means used.

After the second day he is perforce discharged. In most cases his craving is gone, but this generally occurs after a debauch. In fact, the natural history of many cases of periodical alcoholism is that craving ceases after the spree for from one to nine months.

On being discharged, the patient is given:

V. ℞ Tinc. columbo..	oz i
Tinct. capsici..	m xv
Tinct. nucis vomic..	oz i to oz iss.
Apomorphinæ..	gr. 1-3
Tinct. cinchon. co..	ad oz iv.

M. Sig.—Dram i. t. i. d. in water after meals.

The patient is told to take this and report weekly. After a month he reports monthly, and is kept supplied with the bottle, which he is told to take the minute any craving develops, and report at once.

By the application of this method to alcoholics we are able to send out many patients with hope in the future, confidence in themselves, and a staff upon which to lean in this weakness. The same treatment, when applied to patients with the morphine habit, has to be given much longer, and sometimes must be modified by adding bromides, or gradually reducing the morphine. Dr. Brown, the house surgeon, was able in one case to stop immediately the use of morphine taken to the extent of thirty grains a day. The patient did not suffer in the least.

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CLINICAL REVIEWS.

CONGENITAL TEETH.

By E. S. McKEE,
Cincinnati, Ohio.

February 26, 1896, the author was called to see Mrs L. a IIpara in labor at the eighth month of gestation. Twins, both male, were soon delivered, the first presenting the vertex, the second a shoulder which necessitated turning. One was found to have the two inferior incisor teeth present, both children being small and poorly developed; the mother was a small woman. Both teeth were loose and one disappeared at the tenth week. The child died at the twelfth week; the remaining tooth is still in my possession It died suddenly and away from home, the physician attending ascribing death to meningitis. Congenital teeth are very rare. Paris Maternity reports out of 17,578 births between 1856 and 1863 only three cases of congenital teeth; Ballantyne, in the Edinburg Medical Journal for May, 1876, reports 70 cases gleaned from the literature.

Premature labor, as in this case, occurred in that of Crausius at the sixth month; Helwig at the seventh; also that of Lamm, which is reported as occurring prematurely. The extraction of teeth in the mother during pregnancy was reported by Flesh. In the writer's case two teeth were extracted at the third month.

The child was exhibited soon after birth to a number of medical men and about 100 students of the Medical College of Ohio. The tooth remaining at the time of the child's death was removed and was exhibited by the writer to the Cincinnati Academy of Medicine. The case itself had been presented before the Cincinnati Obstetrical Society at its April meeting; none of the gentlemen present at these societies had ever seen an instance of this kind.

The important writers on this subject are Jacobi, Forcheimer, Pliny, Bartholin, Magitot, Schuerig, Ballantyne, Pierce, Vargas, Buist, Mackenzie.

This freak of nature has been noticed at wide intervals and with great rarity for a long time. Shakespeare refers to it frequently. In "Richard III." the Duchess of York says: "Mary, my uncle grew so fast that he could gnaw a crust at two hours' old. 'Twas full two years ere I could get a tooth."

Queen Margaret: "That dog that had his teeth before his eyes."

King Henry VI.: "Teeth hadst thou in thy head when thou wast born to signify that thou camest to bite the world."

Richard: "For I have often heard my mother say I came into the world with my legs forward. The midwife wondered and the women cried, 'O Jesus bless us, he is born with teeth,' and so I was, which plainly signified that I should snarl and play the dog."

In English and French history we have mention of Richard III., Louis XIV., Richelieu, Mirabieu and Marazin. Inheritance is shown in some cases. In the instance of Mattei, the infant's mother had been born with a tooth, and in Limrick's the mother had two congenital teeth, as did also her second child and a child of her sister.

The etiology of premature eruption of the teeth is considered to be due to some abnormal development of the bone. Probably most cases have some connection with Rickett's. In some children who cut their teeth young the fontanelles close early, but not so in those cases where the teeth are congenital. The enamel is usually very thin or absent. The etiology of congenital teeth is in some instances described as the premature occurrence of the processes which normally lead to the cutting of the milk teeth; in a few instances it is probably due to true ectropia of the dental follicles and its tooth. Such premature eruptions are usually found in children suffering from improper nutrition or other abnormal systemic conditions.

Treatment.—A tooth dangling uselessly and aimlessly in the mouth should unquestionably be removed; otherwise inaction is preferable. The teeth are sometimes so situated as to prevent closure of the mouth, or make nursing painful and futile and endanger the nourishment or life of the child. The danger of hæmorrhage is probably magnified by Magitot. Having lost one child, he lays down the rule to never extract the teeth. The hæmorrhage occurred on four separate occasions and proved fatal despite all treatment. If the child is puny and delicate, perhaps syphilitic hæmorrhages are possible. Some writers claim the possibility of no milk-teeth appearing to fill the vacancy. These cases serve as a curiosity for medical men and students, a sight many do not get to behold in a lifetime.

Dr. C. C. Surber, of Independence, Kan., writes me that six years ago he delivered a lady of a nice boy baby who had two central inferior incisor teeth ⅛ inch long and very sharp, but loose. The doctor removed them at four weeks, because of the cutting of the upper gums. He has not seen the child since it was four years old, but the incisors had not then made their appearance. He presented the teeth to Dr. S. W. Kincaid, of Beloit, Kan., who presumably has them now.

Dr. Geo. W. McCoy, Columbus, Ind., reports to me by letter that on December 27, 1874, he delivered Mrs. M. of a male child, natural in every particular except two teeth in the upper gums, one full length and size of temporary teeth, the other small and about half through the gums. Both teeth were exfoliated at fifth week. The child is hearty and strong to-day.

CLINICAL OBSERVATION UPON TRIONAL.

By Dr. J. B. BUSDRAIGHI,

Formerly Assistant to Prof. C. Lombroso.

Among the many hypnotics now at our disposal, those which occupy the most prominent place in medical practice are morphine, chloral, sulfonal, tetronal and trional.

I deem it entirely useless to treat of morphine or of chloral, not only because every treatise upon medicine and pharmacology and materia medica studies them extensively, but also because any practitioner of the slightest experience possesses personal knowledge in regard to them both.

The same cannot be said in regard to trional since the investigations concerning it are recent and as yet somewhat limited in number.

Another remedy that, in late years, has enjoyed an excellent reputation as a hypnotic, is sulfonal; notwithstanding this, a fairly large number of objections have of late been brought to bear against it, some of them quite justified while others have been exaggerated, at times probably for the purpose of giving greater importance to some similar preparation. We have employed it upon an extensive scale, both in several clinics and in private practice, and truth compels us to state that it has rendered us most valuable services.

Among other defects of sulfonal must be mentioned its action upon the blood, which prevents our giving it to anæmic subjects. It also shows an irritant action upon the kidneys, as the urine becomes red (hæmatoporphyrinuria), compelling great precaution when it is employed in people in whom these organs are affected, as in the old, or in convalescents from acute febrile diseases, or from acute or chronic intoxications.

Lastly our literature registers cases of poisoning by sulfonal, some of them followed by death.

Hence we lacked a remedy possessed of the same or of superior efficacy to sulfonal, and without its faults. From our personal experiments we may assert that it may be found in trional.

This is a white transparent substance, of slightly bitter taste, soluble in 320 parts of cold water and a little more readily soluble in hot water. It may be administered in a watery solution, in cachets or in other vehicles, in a daily dose of $\frac{1}{4}$ gramme.

We sought to employ it in cases in which sulfonal had failed or caused some disturbance. Our investigations bore upon ten subjects; four children less than ten years old, three men and three women. I will describe them briefly:

1. Chorea. Boy seven years old, poorly developed, anæmic, with all the characteristics of his disease. Cured in twenty-five days with static electricity and one gramme of trional daily.

2. Bacillary Meningitis. Girl five years old, with muscular atrophy, vomiting, insomnia and nocturnal agitation. Trional caused sleep and quiet.

3. Rachitis in a girl four years old, profuse sweats, diarrhœa and insomnia. Same result.

4. Convalescence from scarlet fever with parenchymatous, nephritis, albuminuria, and insomnia, which was overcome by trional.

5. Cerebral apoplexy. Man sixty-three years old, with degenerative hemiplegia, much troubled by insomnia. Trional was given in two gramme doses.

6. Chronic poisoning by copper in a man, aged fifty-five, through abuse of a remedy containing this metal. Asthma, nephritis, cystitis and nocturnal agitation with insomnia. Trional proved effective in two gramme doses.

7. Neurasthenia in a man thirty-three years old, with the characteristic signs of this disease and marked insomnia. This was relieved by two gramme doses of trional.

8. Hysteria in a young woman of twenty-seven, characterized by frequent convulsive attacks and nocturnal agitation with insomnia. She improved markedly with the same doses.

9. Nervous disturbances of pregnancy in a young woman aged twenty-four, with insomnia and suicidal tendencies. Excellent results with the same doses—two grammes.

10. Anæmia in a young girl aged nineteen, with great debility and insomnia. Trional gave her quiet sleep.

We would have liked to describe the cases at greater length, but preferred to speak of the results obtained.

Trional was well borne in all the cases in which we employed it, showing a prompt and safe hypnotic action.

It occasionally happens that a patient is unable to take it as it may cause nausea, and, in rare instances, vomiting. For this reason it is best to give a fairly active purgative before the trional, which must only be administered an hour or two after eating.

It is also necessary to select light articles of food, avoiding salads, salt fish, dried fruits, etc., and especially iced or alcoholic drinks. It may be given, as we have already suggested, in wafers; it is good practice, however, to follow it with a couple of cups of infusion of linden or of chamomile, quite hot.

The use of trional may be continued for a long time, since harmful effects are never, or at least very seldom, observed. In all cases it is well to give at the same time some of the gaseous mineral waters, such as Seltzer or Apollinaris. In Spain such waters are abundant in nearly all the provinces.

Sleep occurs about half an hour after having taken the medicine; it is quiet, placid, perfectly similar to physiological sleep; the pulse and respiration, which before were restless and nervous, are quieted. It lasts for six or eight hours, giving the patient a sensation of well-being obtained with no other hypnotic.

In the second case the effects were not as evident as in the following ones, as might have been supposed when dealing with so serious a disease as bacillary meningitis. Still it was beneficial and well borne.

In the fourth and sixth cases, in which we had to deal with renal lesions, the trional was perfectly well borne, and, it being our custom to examine the urine, we failed to find any sign to disprove this statement.

In neurasthenia and the nervous disturbances of pregnancy we had already given chloral and sulfonal with negative or with harmful results; trional on the contrary gave satisfactory results.

Even in anæmia it was given with success, being well tolerated and without the phenomena observed with sulfonal.

From what we have stated, we conclude that trional is the best hypnotic known, and that it may be prescribed both in children and in weak and nervous persons with confidence in the results and without fear of damaging after-results.—Correspondencia Medica, No. 25, '96.

EXTRA-UTERINE PREGNANCY.

This is a case of extra-uterine pregnancy. The patient, aged thirty-four years, has had four children, the youngest being about eight years of age. Three years ago she had a miscarriage at about the eighth month and the fœtus had probably been dead a week or more. She menstruated last the 6th of February. In March she did not menstruate. She suspected she was pregnant, but had no bad symptoms until the 21st of March. At that time she was taken with a sudden and severe pain in the abdomen and was confined to bed for several hours. About the time she began to think of sending for a physician the pain subsided and she did not send. Two days later she had another attack of pain, but not so severe. She then went five days, to the 28th of March, when she had another severe attack in the night which lasted several hours. On the 30th she had a still more severe attack and I was sent for. I saw her late in the evening. There was profound shock. She complained of severe pain; the extremities were cold; the face was cold; the pulse was very fast and thready and I felt sure she would not live long. I gave her a hypodermic injection of morphine and by morning she had rallied and was fairly comfortable. She was taken the next day to the Good Samaritan Hospital. Two days after going to the hospital she passed some little decidual masses. The diagnosis from the history alone was unavoidable. For years she had not missed a menstrual period until the one in March. The attacks of pain and the shock were characteristic; and finally, the expulsion of decidual masses was almost unnecessary confirmation. The physical examination did not reveal much except the uterus seemed rather fixed and there was a great deal of tenderness on the right side. The operation was done on the fourth of April. When the abdominal cavity was opened it was found filled with blood, mostly of a light chocolate color, rather thicker than normal. All through the abdominal cavity there were black clots, showing there had been an early hemorrhage and one later. Rupture of right tube had occurred and it was adherent to the omentum.

The patient did very well for twenty-four hours. After that period she began to develop a rapid pulse and the temperature went up. The temperature, which had been 101° for twenty-four hours after the operation, went up to 102½° about thirty hours after the operation and the pulse became very rapid; at one time it was 156. She had lost control of the sphincters and there were involuntary discharges from the rectum and bladder. As she had been purged pretty freely, and as there was no tympanitis, one-fourth of a gr. of morphine was given hypodermically. By the next morning these symptoms had cleared up and her pulse had come down to 120, and she has since progressed without trouble and is making a good recovery.—Proceedings of Soc. Am. Soc.

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MEDICAL NEWS AND NOTES.

TWO CASES OF RECOVERY FROM PERNICIOUS ANAEMIA.

A Russian Army-Surgeon, Dr. M. B. Blumenau, has observed at the military hospital of Grodno a case of pernicious anæmia, which was cured with remarkable rapidity and completeness by the ingestion of bone-marrow. The patient was a soldier, twenty-three years of age, in whom the anæmia had supervened as the result of severe dysentery. Among other symptoms, he presented malleolar œdema, ascites and a certain degree of dilatation of the right half of the heart, with a systolic murmur at the apex. He was so weak as to be unable to rise. No favorable effect having been obtained by the administration of large doses of iron and arsenic, Dr. Blumenau ordered the patient 90 grammes daily of raw-bone marrow, which he took either on bread or soup. This treatment, without any other medication, was followed by the disappearance within two months and a half of all the morbid phenomena, including weakness, œdema, ascites, and cardiac disturbances.

On the other hand, Dr. G. Dieballa, assistant of Dr. K. Kétli, Professor of Clinical Medicine at the Medical Faculty of Budapest, has had under observation a patient, fifty years of age, suffering from progressive pernicious anæmia of unknown origin (neither adult parasites nor eggs having been found in the stools), which was not relieved either by bone-marrow, iron, quinine, arsenic or inhalations of oxygen. These various substances had been administered one after the other for five months, but in spite thereof the weakness and emaciation of the patient steadily increased, and ultimately attacks of delirium supervened. Salol was then resorted to on the assumption that the disease might be due to auto-infection of intestinal origin, 1 gramme of this drug being given five times daily. Within a week the attacks of delirium ceased, the patient recovered his appetite and gained strength to such an extent that he was able to rise, which he could not do before. This improvement continued as long as salol was given. Twice an attempt was made to suspend the treatment, but each time there was a marked recrudescence of the symptoms. The administration of salol was then continued, at first at the rate of 5, and subsequently 3 grammes daily. Three months later, recovery appeared to be definitive and the treatment was therefore discontinued. At this time the patient had completely regained his strength, and the physical signs of anæmia (pallor, bruit de souffle over the heart, enlargement of the liver and spleen, and characteristic changes in the blood) had disappeared.

Dr. I. M. Love has resigned his professorship in the Marion Sims College of Medicine.

Dr. William Thayer Smith, Professor of Physiology in Dartmouth College, has been appointed in the place left vacant by the death of Professor Carleton P. Frost.

The Paris Cour d'Appel has recognized syphilis as a reason for granting a divorce.

A New York journal claims that the death rate in Chicago is easily kept low, not by "the attention to sanitary measures, but by the estimation of the population at a much higher figure."

A Colorado journal states the following little problem as its first editorial for September, 1896. Given a young woman who says she is married, but whose husband is away at the time of the birth of her babe, that said woman refuses to nurse her babe, and that the babe wastes under artificial foods, and, as soon as the woman is able to be up and about apparently neglects the child so that the babe dies. What could be suggested in a practical way to save the life of the child? It seems to us that there are several little problems, none of which are particularly easy to solve. A babe would be apt to waste away under artificial foods that did not agree with it, and more apt to do so without any at all. Probably the foundling asylum would be the safest refuge for the unwelcome stranger.

In an editorial in the New York Medical Journal there is a brief résumé of the communications made by Dr. Juan de Dios Carrasquilla to the Academy of Medicine of Bogota, touching his experiments and results in the serum treatment of leprosy. "He first bleeds a leper, using an adult whose general condition is fairly good, the blood drawn varies in amount from 100 to 150 cubic centimeters. It is received into a sterilized vessel and carefully covered, kept away from the light, and, above all, kept perfectly quiet. In from twelve to twenty-four hours a superficial layer of the serum, that only which is perfectly limpid, is removed with a pipette. If it is to be kept for some time before it is to be used, it is passed through a layer of powdered camphor contained between two layers of cotton, to preserve it, and it is kept away from light and heat. Thus prepared, the serum is injected into an animal that is refractory to leprosy, preferably a healthy young horse in good condition. Roux's method of procedure is employed." The two points of greatest importance and at the same time of most difficulty to determine, are the amount of serum to be injected at one time and the interval that should elapse between injections. He believes that 45 cubic centimeters is the proper medium dose, and should be given at intervals of ten days. The horse is bled in from five to ten days after the last injection, preferably from the jugular vein; the Nocard Roux process is followed for obtaining aseptic horse serum, and it is treated in the same way as the human serum. The dose of the serum for subcutaneous use on the human subject is from 1 to 5 cubic centimeters, according to the strength of the serum, the constitution, age, and other circumstances of the patient, the variety of the disease, and so on. The locality preferably for the injection is just to the outer side of the trochanter major. Dr. Carrasquilla has great hopes for the success of this method of treatment.

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REVIEWS.

Reference Book of Practical Therapeutics, by various authors. Edited by Frank P. Foster, M.D., editor of the New York Medical Journal, and of Foster's Encyclopedic Medical Dictionary; in two volumes, Volume I. New York: D. Appleton & Co., 1896.

"The leading idea in the preparation of this work has been to make it permanently serviceable to the practicing physician. It is not intended to take the place of treatises on Materia Medica or on pharmacy; hence, as regards the physical properties of drugs, their mineralogical and chemical potential or zoölogical relations, and so on, the endeavor has been to set down only so much as is of direct bearing on the use of those drugs in medical practice, or on the management of poisoning due to them." Such being the declared purposes and scope of the work, it remains for us to speak of its arrangement and of its appearance. Everything is arranged in alphabetical order, volume one beginning with A. B. C. Balsam, and ending in Myrtol. The name of the editor is a sufficient guarantee that the work is encyclopedic in character and entirely worthy of its title. An illustration of the "up-to-date" character of the work may be gotten from the several short articles on serum-therapy and antitoxines. The question of hydriatics or hydro-therapeutics receives extended consideration; so does, also, the question of exercise, and, indeed, it seems to us that the various authors have given a deservedly large amount of consideration to the therapeutic measures most easily available to the whole profession. For instance, climatic treatment is unusually full. The various authors have not hesitated to give accounts of drugs that are proprietary and patented, where these drugs have proved themselves to be of especial service in the treatment of disease. The article on hypnotism is exceedingly interesting, and the author states, "It is no longer necessary in writing on hypnotism to waste words in discussing whether the condition is real or imaginary. Every careful conscientious and scientific observer, who has thoroughly investigated the subject, has been convinced of its genuineness." He adds in conclusion, "I wish to reiterate that while I believe that hypnotism properly used has a place in medicine, it is a rather limited one." Our own belief is that until more is definitely known of the process, it is an exceedingly questionable method of therapeusis. The book, as far as it goes, justifies the claims made for it, and it will unquestionably find a place in the library of many physicians.

A System of Gynæcology, by many writers. Edited by Thomas Clifford Allbut, M.A., M.D., LL.D., F.R.C.P., F.R.S., F.L.S., F.S.A.; W. S. Playfair, M.D., and LL. D., F.R.C.P. New York: The Macmillan Co., and London, Macmillan & Co., Limited. 1896. Price, $6; with Allbutt's System of Medicine, $5.

GAILLARD'S MEDICAL JOURNAL.

A MONTHLY JOURNAL OF MEDICINE AND SURGERY

Scientia et Veritas Sine Timore.

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EDITORIALS.

SALUTATORY.

To one and all we tender our greetings and our best wishes for a bright, happy, and prosperous New Year. While the one that has just passed may have for individuals been laden with blessings and benefits, it certainly has not brought to the whole body of the profession the full measure which is their due. Indeed, it has in this respect been not less unkind in many ways to medical men than it has to the public at large. Financially, the year has been one of trouble perhaps to more classes of people than any year since '73. Physicians probably suffer more in times of financial distress than any other class of people; first, because bills, long since due, which under ordinary circumstances might be collected, under such circumstances are absolutely unavailable; in the next place, as every druggist knows, people will not in hard times consult a physician until the very last moment; in good times they never pay them—granting, of course, a number of notable and worthy exceptions—until there seems to be absolutely no excuse for not so doing. Again, in cities with abundant dispensary facilities, many who are still able to pay a physician feel justified in availing themselves of free medical treatment in any way that they can obtain it. And so it is in many other ways that we need not mention in detail, that the medical profession during the past year has, from a financial standpoint, been a severe sufferer. On the other hand, we are justified in stating that they have not in the least degree shown any less disposition towards the relief of suffering, nor have they in any way retrenched their own expenditures of money, time and zeal, in the addition of knowledge useful to the profession and to the public at large. The triumphs of the year are comparatively few, but the far-reaching possibilities of a single one in the relief of human suffering and in the reduction of infant mortality, outweighs and outclasses any other accomplishment of which we have knowledge.

The possibilities of the X-ray have been by no means developed, but its value as an accessory to accurate diagnosis cannot be doubted. Bacteriology, along the lines of preventive and curative inoculations, continues to open up a wonderful mine for working and discovery. The white corpuscles have assumed new potentialities. Sir J. L. says "that upon investigating, influenced by the researches of the Russian naturalist and pathologist, Metchnikoff," he observes, "that, after passing through the walls of the vessels, they not only crawl about like amœbæ, but, like them, receive nutritious materials into their soft bodies and digest them. It is thus that the effete materials of a tad-pole's tail are got rid of, so that they play a most important part in the function of absorption." "But still more interesting observations follow. He found that a microscopic crustacean, a kind of water flea, was liable to be infested by a fungus which had exceedingly sharp pointed spores. These were apt to penetrate the coats of the creature's intestine and project into its body cavity. No sooner did this occur with any spore, than it became surrounded by a group of cells, which are contained in the cavity of the body and correspond to the white corpuscles of our blood. These proceeded to attempt to devour the spore, and if they succeeded in every such case, the animal was saved from the invasion of the parasite, but if the spores were more than could be disposed of by the devouring cells (phagocytes, as Metchnikoff termed them), the water flea succumbed. Starting from this fundamental observation, he ascertained that the microbes of infective diseases are subject to this same process of devouring and digestion, carried on both by the white corpuscles and by cells that line a blood vessel; and by a long series of most beautiful researches he has, it appears to me, firmly established the great truth that phagocytosis is the main defensive means possessed by the living body against the invasion of its microscopic foes."

While chemistry has furnished us with a number of new combinations for the treatment of disease, the drugs that have proved their right to live and to be considered of especial value in the practice of medicine are extremely few. Indeed, each year of practice seems to emphasize the teachings of many years ago, that the necessary drugs are comparatively few, that the additions to a short list are made at long intervals; but the year, if it has not added a large number of new drugs, has proved again the value of some of the old ones.

The work in special lines, eye, ear, throat, and so on, and so on, has been prolific in many ways. Abdominal surgery has received an unusual amount of attention, while many other surgical branches have been neglected.

It shall be our effort in the future, as it has been in the past, to present to our readers in a form which can be put into practical service the best recent information, not only as to what is new, but also as to what is old. We again extend to all who may read the JOURNAL the invitation offered in our issue of March to assist us in this work. If we have an apology to offer, it is simply that we are human. If our readers are dissatisfied and remain silent, we have no way to know it. Again, we extend the compliments of the season.

WHO ARE SPECIALISTS ?

Several of the journals are not improperly considering the question of specialism and specialists. The tendency in the last few years has probably not as well marked as formerly in the direction of producing expert specialists in a great many lines. It is not necessary to take up the question of the real value of the true article, nor to call attention to the fact that probably the greatest desideratum in medicine to-day is the first-class all round practitioner; but it does seem only fair to note the fact that many of the men who pose as specialists do so with great injustice to other members in the profession. It is not claimed that any man should under all circumstances be absolutely restricted in the practice of his profession; but when a practitioner comes into a case as a specialist it does not increase our respect for him to see his efforts, at times successful, to treat the whole family for any trouble that may thereafter obtain. We do not believe that the lines are drawn closely enough in this matter, and if the profession does not protect itself, it can hardly look elsewhere for relief. The matter is simple enough. A man of the type we have described, who claims to confine his work to an individual line, and who is known not to do so, can easily be prevented from practicing such a deception upon the same practitioner a second time. Where a man proposes to be an eye specialist, and it is known that he does general work, it is an easy matter not to refer any eye cases to him.

Where a man claims to be a Simon-pure surgeon and treats contagious diseases and takes obstetric cases, it ought not to be difficult to find somebody else equally competent to do the surgical work. And so we might go through the various specialties, pointing out inconsistencies and irregularities. We are aware of the fact that in many places it would simply be impossible for a man to pose as a genito-urinary surgeon specialist, and, at the same time, take in a general practice. If the profession would protect itself in such matters, it would soon find honors and emoluments more evenly divided.

CONFLICTING EXPERT TESTIMONY.

An editorial in one of the daily papers, casting severe reflections upon the whole profession, and doing great injustice to all medical men, brings us face to face with a problem by no means easy of solution. There are so many factors entering into the giving of expert testimony, that it is out of the question to try to follow out each detail that presents itself. But there is one factor of prime importance which cannot be disregarded, namely, that every man takes an oath that he will tell the truth to the best of his knowledge and belief. We do not believe that medical men, who are of all others known to be honorable and upright men, have a complete change of faith, knowledge, and opinion co-incident with the remuneration paid for their services. It is perhaps not easy for the layman to understand that there may be material differences of opinion—absolutely honest ones—between the best informed medical men. Over and above his books and his teachers, every man must be guided in his opinions by his own individual experience. Not only so, but this experience may cause him constantly to change his own opinion. Some years ago a lawyer read to an expert witness an extract from a book recently published by the witness, and asked him if it was true; to which he received the reply that at the time it was written, the opinion of the writer was as stated: his experience since has compelled him to change that opinion. As a matter of fact, sooner or later, we must come to a separate hearing of the medical side of a case, a hearing before men who are capable of understanding, not only the questions, but the answers; in short, before a body of medical men; a plan which has already been suggested by a prominent member of the profession, and has since been under the consideration of eminent legal men. Until this is done, the ridiculous and absurd comments in regard to medical testimony must be expected. It would be quite as reasonable to testify in Chinese and expect an intelligent appreciation by a Russian audience, as to give expert testimony in medical terms in answer to questions often absurd, if not impossible, before a jury of laymen.

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ORIGINAL ARTICLES.

A FEW BRIEF REMARKS ON THE PATHOLOGICAL ANATOMY OF SOME OF THE MORE PRONOUNCED NEUROPATHIC DEGENERATIVE CHANGES NOTED SUBSEQUENT TO SEVERE INJURIES OF THE JOINTS AND EXTREMITIES.

By THOMAS H. MANLEY, M.D.,

Professor of Surgery in the New York Clinical School of Medicine.

Any one who has had a moderate experience in severe injuries of the joints or the extremities, must have been impressed, in a considerable number of this class, by the marked atrophic changes so commonly observed in the muscle structure, and all the tissues, not only in the immediate vicinity but in neighboring organs, and even in distant parts.

As the degree of consecutive degeneration and impairment in function depend, in certain circumstances, on other factors than physical force, it becomes a matter of some importance to comprehend what the essential pathologic changes are.

In circumscribed motor-atrophic paralysis, certain definite and well-marked pathological changes occur which involve the entire limb, a segment of it, a group of muscles, a single muscle, one, or more articulations, or all the component elements in totality. Much will depend upon the amount of damage originally inflicted, and whether the disease pursues an acute or transient course. To clinical observation and pathological research we are mainly indebted for our knowledge of the physiological properties of the multipolar-ganglia in the anterior cornua in the spinal-medulla. Indeed, physiologists have generally denied that there are distinct trophic centers, or that they constitute a distinct, independent system. From an anatomical standpoint, their position cannot be yet assailed. But pathologists have clearly proven that such a system does exist, rather, however, by induction, than by an anatomical demonstration; and Charot, with Myhem, Aran and many other independent observers, have each reported instances of isolated motor-paralysis with absence of marked atrophic changes. The converse has been abundantly proven; also, viz., that cases of progressive muscular atrophy without any motor impairment, except such as we may expect from structural damage to the muscle elements in advanced cases, are sometimes witnessed.

The phase of paralysis which we meet with in a limb or segment of it, resulting from local disease or injury, is always of a reflex character, as the functional vitality of the central cellules preserves its integrity unimpaired; and degenerative changes are limited wholly to peripheral districts.

First, the limb is swollen, hyperæsthesic and helpless. It is inflamed. As far as the muscle itself is concerned, the inflammatory changes at first are limited to the cellular tissues only, the sheaths and sarcolemma. It is, as yet, purely interstitial. Now, provided the nerve itself has not sustained serious injury, the patient is healthy, and appropriate treatment is instituted. The striæ, the contractible elements are not compromised, hyperplasia of the fixed and other cells of inflammatory change, disintegrate and are resorbed when structural and functional restoration is complete. But when the muscles and nerves have suffered very great violence by distention, laceration or contusion, resolution may never take place; or, if it does, it will be incomplete; in which case there is parenchymatous atrophy, and the head of the bone, if the lesion involve the hip or shoulder or other shaft, may spontaneously luxate, through want of muscular support. The joints which are covered by muscle have so shrunken as to indicate that the cellular tissues and subcutaneous fat have wasted, and, hence they appear greatly enlarged. This constitutes the most exaggerated phase of this lesion. Those which most concern the surgeon are the cases of a less aggravated type, in which but one major articulation perhaps, or but one large muscular district is implicated. In these cases, in which motor-palsy is present with neuro-trophic changes, a most complex pathological condition commonly prevails.

Resolution failing to dissipate the products of inflammatory changes, in the connective tissue elements, we will now witness their spread by propagation directly into the muscular cellules, or the contractile substance proper, when we are in the presence of parenchymatous inflammation.

When parenchymatous changes involve an entire muscle, or group of muscles, interesting phenomena present themselves. One of the first and most notable changes in the elementary structures is seen in the primitive muscle fascicules. The protoplasm in the muscle corpuscle, from which, normally, muscle fiber is nourished, now diminishes in quantity, as it is compressed by the advancing interstitial hypertrophy which encompasses it in every direction. As this pathological change extends, a parenchymatous degeneration is developed, and we meet with a true sclerosis, or cirrhosis of an organ's cellular elements. This fibrous infiltrate, is at first, hypertrophic. That is to say, the muscle suffers no diminution in volume; in fact the limb, in the first stage, is rather fuller and more plump than normal. But as we pass the hand over it, we will note a marked change in the outline and consistence. The parts may have a soft, flabby peel, or, at the ends of the shafts, where certain sets rise, the skin, cellular-membrane, muscle and bone are bound together by an inflammatory infiltrate which has undergone fibro-genesis and consolidation. The motor-paralysis is commonly in strict proportion to atrophic waste, and always antecedent to it.

Vascular-stasis, a diminution of animal heat, a turgescent or œdematous state of the subcutaneous tissues, with a diminution or entire loss of joint action is usually present when a limb is the seat of subacute or chronic atrophic-motor paralysis. At the beginning, and during the course of this condition of parenchymatous inflammation of muscle, important molecular and cellular changes are observed when the degree and quality of function necessarily resulting must depend on their rapidity and extent. When a connective tissue proliferation, with a myositis which fails to resolve, involves a muscle or entire group of muscles, the fibrillæ within the sarcolem ma undergo a complete transformation in their minute elements. The cell elements undergo gliomatous, lipomatous, calcareous, or fibrous degeneration. This may involve but isolated fasciculi of a single muscle, a single group of muscles, or spread to all the muscles of a limb. Contemporaneously with these degenerative organic changes in the muscle, the adjacent contiguous ossific and arthritic structures participate.

As the full nutrition of a movable bone or joint can be maintained only by ample exercise of healthy muscle by unimpeded circulation, the bone suffers interstitial changes, becomes less vascular and immovable by protracted immobilization. As the muscles shorten and contract, an articulation becomes fixed; and and its surfaces are brought together by such pressure as to excite a synovial inflammation, the effusion of which, in time, undergoing plastic, fibrous or osseous changes; so that, we will have, 1st a muscular-anchylosis; 2nd fibrous, and, 3rd osseous.

The sensory nerves, for some unaccountable reason, tolerate traumatisms with much greater impunity than the motor, as we so often see demonstrated in those cases in which motion is in abeyance; yet the limb is hyperæsthetic, and every movement imparted to it elicits the most agonizing pain. It might be alleged, in explanation of this, that the sensory or mixed nerves escape injury. But this will not hold when the motor-filaments of an entire plexus are all involved, or even when one of the very large trunks is compromised; for we know that the decussations and inter-communications of the motor with the sensory branches are both numerous and intimate. "Duplay, Clado, Hyem and other observers, have by their extensive writings on this subject, by their experiments on the lower animal and clinical observations, most opportunely called attention to those myopathies of a degenerative order, dependent on a traumatic origin and local eccentric pathological conditions."—(Raymond Maladies du System Nerveux p. 201—Ibidem p.p. 307-346.)

In accident cases, involving the joints or long bones, litigation commonly follows. In order that no injustice be imposed on the injured, or on the other hand that attempted extortion be successfully combated, it becomes imperative that we familiarize ourselves with the topic which the above brief notes but crudely outline.

PUERPERAL CONVULSIONS FROM THE STANDPOINT OF PREVENTION.*

By JOHN N. UPSHUR, M.D., of Richmond, Va.

Professor Practice of Medicine in the Medical College of Virginia, etc.

A firm conviction of the great responsibility resting upon us in the care of the puerperal woman, and the skillful guidance through the perils of this period to its happy consummation in a safe delivery, is the motive prompting to the discussion of this condition—so frightful in its manifestations and so dire often in its consequences.

I cannot emphasize too much the importance to the patient of an early engagement of her medical attendant, that she may be closely in touch with him, and that he, by *extraordinary diligence*, may be keenly alive to every circumstance which may be to her a source of peril, giving to his patient advice as to personal care (of diet, exercise, rest, bowels and kidneys), making regular, often frequent analyses of the urine to determine the presence of albumin or the lacking elimination of extrementitious solids. Often there is danger ahead when urinary analysis is negative in its results, and the nervous system is ripe for a dangerous explosion as soon as something occurs to excite the requisite reflex. I cite the following cases for sake of illustration:

CASE I.—Mrs. T., a woman of fine physique and robust health, came under my care in 1885; she would never permit me to see her until labor came on, though reported by her husband as being very dropsical. Frequent examination of her urine failed to show any albumin, though the amount of urine was scanty. Treatment through the medium of her husband was of course very unsatisfactory. When labor came on, I found her the most dropsical woman I ever saw at term. She was kept under chloroform until delivered with instruments, after a protracted labor. In four succeeding pregnancies she was never so dropsical again, but each period of pregnancy was filled with symptoms of threatened eclampsia. The patient was a hearty feeder, her urine was usually scanty, albumin apparent on analysis, but not marked.

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*Read before the Richmond Academy of Medicine and Surgery, November 22, 1897.

In her third pregnancy she had an attack of vertigo, followed by a period of insensibility; she has always suffered with her head. Her treatment during each pregnancy consisted of low diet, abundant drinking of lithia water, and keeping bowels in a condition of mild diarrhœa. She has usually taken chloroform during the latter part of the second stage of labor, and has been safely delivered five times without a single convulsion, and is now doing well in her sixth pregnancy, having reached the end of the sixth month.

Case II.—Mrs. W., of delicate build, in her second pregnancy, during the ninth month had agonizing headache, which failed to respond to remedies; saw flashes of light and other objects continually before her eyes. When labor came on, she was kept under chloroform until delivered; her skin was so dry as to feel parched. Repeated examination of urine failed to detect any albumin. Within an hour after delivery, she complained of not being able to see (she had had no undue amount of hæmorrhage) and began to talk wildly and incoherently. Chloroform was given to control excitement, followed by full doses of potassium bromide and pilocarpine; skin acted freely and all untoward symptoms subsided; urine showed large percentage of albumin. Convalescence uneventful until fourteenth day, when marked symptoms of septicæmia developed, but she passed safely through a most dangerous illness. [See *Transactions State Medical Society of Virginia*, 1886.]

Case III.—Mrs. H. had been carefully watched during second pregnancy, urine frequently analyzed, with negative results. About the time she reached the fifth and a half month I was called to see her with a sharp attack of cholera morbus; a day or two of treatment was sufficient to restore her to health. She was discharged with caution as to imprudence in diet, and every other respect. On the evening of the next day she went out to supper ate very heartily of almonds and raisins. I was called early the following morning to see her in what her husband called a *trance*. When I reached her she talked to me rationally, and manifested no symptoms of special gravity. At 4 P. M. she was seized with a violent puerperal convulsion, quickly followed by a second, and for a few moments I thought her dead. I bled her freely from the arm, labor was brought on, and after delivery she made an uneventful recovery—urine for the first time loaded with albumin.

Her next pregnancy progressed satisfactorily to the ninth month, when she complained of distressing head symptoms, urine scanty, but free from albumin; some disturbance of vision. She was freely purged, with calomel gr. vj., croton oil gtt. j., with complete relief of head symptoms. The week following, head symptoms again returned, and the dose of calomel and croton oil was repeated with as prompt relief as at first. Labor came on a few days after, and she was safely delivered, without any complication; convalescence speedy and uneventful.

CASE IV.—Mrs. D., primipara, æt. 22, stout and plethoric. I was retained six weeks before confinement. Frequent analyses negative, except once a trace of albumin, until October 12, when urine was found loaded with albumin, being almost solid on boiling. Was called at 10 P. M. found she had had three convulsions, cervix rigid, dilated size of a nickel. She was bled freely from the arm, manual dilatation persisted in for five hours; child turned and delivered; a serious convulsion during the labor, which looked as though it would be fatal; no untoward symptoms during convalescence. I found that she had been complaining for a week before the labor with violent headache. At the same time she had been eating enormously. I had not been consulted.

CASE V.—Was called in consultation to see Mrs. H., primipara, æt. 22. Saw her at 6:30 P. M. She gave the history of an unusually comfortable pregnancy. Had engaged her medical attendant *two days* before; said she had passed sufficient urine. Awoke at 5 A. M. same morning with severe headache across vault of cranium, extending over occipit and down back of neck, and some abdominal pain, which was supposed to be colic, as her time was not up (280 days) till two weeks later. Had been abstemious in her diet during latter part of pregnancy, abstaining from eating any supper. Just before 3 o'clock P. M. she described a sensation in her head as if blood was trickling through it, and a few moments later a violent eclampsic seizure developed. Her medical attendant, when he saw her in the morning, had ordered $\frac{1}{8}$ gr. doses of sulph. morphia every two hours—she having taken in all about $\frac{7}{8}$ gr. So soon as he saw her after the convulsion, he bled her freely and administered chloroform, controlling the convulsion. When I saw her she was completely relaxed, fully dilated and bag of waters filling the vagina—ruptured accidentally in my examination. Labor progressed satisfactorily, and she was delivered of a live baby at 8:15 P. M.—skin hot and dry, pulse soft, feeble and 120 per minute—profoundly unconscious. Another convulsion at 9 P. M. No secretion of urine since early morning; an enema of bromid. potass., sulph-morph. and camphor was administered, and she passed into a quiet, natural sleep, having a good night with the exception of slight restlessness. She had also a hypodermic of 1-50th gr. sulph. strychnia. On next day she had three more convulsions, but of diminishing severity and longer interval; vision impaired, and she was unable to recognize her friends before the third day; periods of consciousness alternated with periods of delirium.

The baby had one convulsion on the day subsequent to its birth, but afterwards did well.

REMARKS.—It is not my purpose to discuss the classification or causes of eclampsia; this is sufficiently done in all modern text books. It is in the direction of such *care as will prevent the occurrence of convulsions* that I wish to consider the subject.

The cases cited as illustrative, emphasize—*first*, the necessity of early engagement, and subsequent close supervision. Case I. shows how such care warded off trouble in five pregnancies. Case II. points to threatened trouble indicated by the severe head symptoms with negative evidences from the urine, and, with Case V. emphasizes this symptom and the hot, dry skin—relief coming when the skin and kidneys had their function restored. Cases III. and IV. point to the necessity of careful dieting, and the overloading of the stomach as an exciting cause—in the one case no albumin, and in the other albumin only twice discovered prior to the development of convulsions. Where there is excessive eating, a hypernutrition is the result in a system in which there is already a hyperplastic condition of the blood. The attempt on the part of the kidneys to eliminate increased effete matter begets kidney irritation—it fails in its function, and the appearance of albumin in the urine is the external manifestation of poisoning of the system of toxins, which, unless eliminated, result in an eclampsic explosion.

The patient's diet should be nutritious, digestible, not rich; regular exercise in the fresh air, and especial care of the bowels and kidneys.

The administration of saline cathartics should be frequent, making the mucous membrane of the bowels eliminative and derivative; an abundance of lithia water and other necessary diuretics to stimulate the kidneys. If the patient shows evidences of anæmia full doses of the tinct. chloride of iron long continued will be of value.

The treatment of the case when convulsions occur, I believe, consists imperatively in free administration of chloroform and prompt bleeding, with active purgation if the bowels are constipated; nor should we forget that the welfare of the patient depends upon as prompt delivering as possible by manual dilatation of the womb and forceps or turning. When, as in Case V. the patient remains unconscious after delivery, with symptoms of depression, as evidenced by rapid, feeble pulse, a hypodermic of strychnia-nitrate, will do much good by its action in sustaining the heart, and diminishing cerebral congestion by its toning-up effect on the vaso-motor nervous system, and at the same time sustaining the uterus in firm contraction.

I am convinced of the usefulness of morphia in conditions of rigid os and cervix in the first stage of labor—but, if there be symptoms of threatened eclampsia, *it is positively contraindicated, and should not be given* in the treatment of the convulsions; it arrests secretion in the skin and kidneys, and favors the retention of the effete materials in the system.

210 *W. Grace St.*

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TRANSLATION.

UPON THE DIAGNOSIS OF THE INTRA-ABDOMINAL AFFECTIONS WHICH, FROM THEIR ACUTE SYMPTOMS, BELONG TO THE IMMEDIATE DOMAIN OF SURGERY.

Translated for Gaillard's Medical Journal.

By H. McS. GAMBLE, Ph.D., M.D., Moorefield, W. Va.

We shall omit all the intra-abdominal affections that have little by little invaded the wall, and have therein determined inflammatory manifestations.

Much the most important diagnosis to be made is that of the acute intra-abdominal affections which manifest themselves by rapidly grave peritoneal reactions, and in which success depends upon the rapidity of intervention.

Therefore we are going to place the clinician in face of a woman who presents herself before him, with the features drawn, the eyes excavated, with meteorism, acute abdominal pains, a rapid pulse, and in a state of nausea which determines repeated attacks of vomiting. Many affections may present this symptomatic picture with more or less accentuated signs, and it is necessary to differentiate them from one another.

The first point to be elucidated is the following:

Is there or is there not acute intestinal occlusion?

Interrogation of the patient will at once answer this question. In the case of intestinal occlusion, she will say, in effect, that since a given time (twenty-four, thirty-six, forty-eight hours) not only has she had no stools, but also that she has not passed the smallest amount of gas by the anus. (It is very important to insist upon the emission by the anus, for if one confines oneself to asking her if she has passed any gas, she will answer yes, because of the eructations she has had.)

A.—There exists no intestinal occlusion.

The features are altered, the coloration of the integuments is peculiar, it is earthy as in infectious diseases. There are attacks of vomiting as in occlusion, but these are further apart, bilious, porraceous even, but never fecaloid. Some cases are emitted by the anus, and the stools may appear. Meteorism is less considerable. The pains are in general less acute.

When we have to deal with a woman, it is necessary to immediately practice the vaginal touch, which will show whether or not the genital organs are involved. The metro-salpingites, the periuterine hæmatoceles, the inflamed cysts of the ovary, very often supply this cortège of peritoneal symptoms. It will therefore be necessary, by the touch, to diagnosticate these different affections, causes of the symptoms which we have described.

If nothing is found on the side of the uterus or of the annexes, attention must be directed to that of the cœcum. Appendicitis and typhlitis will be recognized by the palpation of the right iliac fossa which almost always determines at that point a more intense pain than in the other points of the abdomen. Palpation permits us also to recognize a peculiar empatement of the region, and sometimes even a tumefaction which leaves no doubt. It must be remembered that the meteorism may be hardly apparent, or even may not exist at all, and that one may even discover some contracture of the muscles of the right side of the abdomen. Past events have a great importance in this diagnosis. They will reveal the existence of one or several anterior painful attacks. They will show the bad state of the intestine manifesting itself by diarrhœa and by bad digestion. They will show finally the frequently sudden commencement of the attack which succeeds an effort to lift a weight, or which shows itself after a repast more copious than usual.

If the attentive examination of the cœcal region has yielded no information, one will have to think about a more or less circumscribed form of peritonitis.

Among the latter, tuberculous peritonitis ought always to be looked for, for it is capable, in its acute form or during an exacerbation, of giving rise to the peritoneal reactions that we are now studying. The slowness of the evolution, the presence of ascites, the nature of the pains, which are less severe, and the general state of the subject will enable us to make the diagnosis.

Finally, it is important to understand that the infections of the liver are capable of giving rise to perihepatitis which betrays itself also by peritoneal symptoms and to examine the liver with great care, when the pain has made its début at its level, and when we do not find elsewhere the cause of the symptoms which present themselves.

B.—There is an intestinal occlusion which is manifested by other signs than the arrest of fecal matters and of gases. The vomited matters that were at the commencement alimentary or bilious are not long in becoming frankly fecaloid, and the meteorism is considerable. There is therefore an obstacle to the course of the intestinal matters; this obstacle, must be sought for, but first, it must be understood that there are intestinal paralyses marked by peritonitis which produce what is called pseudo-occlusion, the false intestinal occlusion. Peritonitis by perforation is a type of it. This latter will be distinguished from true intestinal occlusion, by the following characters: the vomitings, although capable of becoming fecaloid in peritonitis by perforation, assume this character less commonly and may remain bilious for a long time. In a certain number of cases, the patients have been able to pass some gas by the anus. The meteorism is less considerable than in occlusion. The sonorousness does not occupy the whole extent of the abdomen, and, as Duplay has pointed out, there sometimes exists in the regions of the flanks, in the hypogastrium, some flatness, an indication of a peritoneal effusion; one also sometimes finds the bruit hydroaérique (see exploration of the peritoneum). Besides, the symptoms make their début by an extremely severe pain, which may be readily localized at a point of the abdomen, a character which but rarely presents itself in internal strangulation. Finally, the thermometer may afford some indications by showing in the case of peritonitis, a more or less decided elevation of the temperature, which remains normal, or which falls in true intestinal occlusion.

The history of the disease will put us upon the right track. We may be in presence of a perforation caused by typhoid fever, or due to an ulcer of the stomach; but in the majority of cases, we will have to deal with a perforation of the appendix, which will be suspected by the possible existence of anterior appendicular colècs, by the suddenness of the attack seizing the individual, so to speak, in full health, by the localization of the pain, on the right side, and also by the age of the subject.

Having recognized the fact that one had encountered a true intestinal occlusion, after having, be it understood, eliminated the accidents of any poisoning or symptoms of cholera, it is necessary to make every effort to ascertain the variety of acute occlusion before which one finds oneself.

One will commence by carefully examining all the openings capable of giving place to a strangulated hernia: the inguinal, the crural rings, the umbilicus and the linea alba: in the region of the inguinal canal, one will carefully palpate the parts, remembering the possibility of an interstitial hernia, above all if the patient is cryptorchide, the examination will be conducted with the view of the possibile reduction en masse of the hernial sac and its contents, particularly if taxis has been practised. Finally, one will practise or will have practised by an assistant, because of the possibility of intervention, a careful rectal examination by touch, which will denote the absence of any obstacle in that quarter, whether it be imagination, or contraction; this done, every effort will be made to diagnosticate the variety of intestinal occlusion presenting itself. Is it an imagination, a volvulus, a strangulation caused by a band, by a diverticulum, or by an accidental ring?

Imagination alone, as Duplay has authoritatively remarked, can be recognized with some degree of certainty by the following symptoms: the occlusion is generally incomplete and the distention of the belly is inconsiderable. There are often sanguinolent stools, formed of a mixture of mucus and of glairy matters; these stools may be accompanied by tenesmus. In many cases, palpation of the abdomen will allow us to determine a soft tumor, painful to pressure, slightly mobile and presenting the form of a sausage. Finally ileo-colic and ileo-coccal imaginations, are more frequent moreover, the absence of the cœcum and of the ascending colon in the right flank will be noted and a certain depression of the iliac fossa of the same side, which contrasts with a more or less voluminous prominence of the left side. Imagination is peculiarly frequent among children.

Volvulus will only afford presumption. It is very often seated upon the sigmoid flexure and is principally met with in adult life and in old age. Wahl claims that the twisted knuckle is enormously dilated and that palpation, under anæsthesia, permits us to recognize it. It has also been said that the attacks of vomiting were delayed but a little while. All these signs are of the most doubtful character.

We will say the same for internal strangulation, whether it be caused by a diverticulum, by an accidental fibrous ring, or by a bridle. It may be suspected from the acuteness of the pain, the suddenness and the rapid appearance of the symptoms. Besides attention may be directed to strangulation by band, when we shall discover some peritoneal trouble in the past history of the patient, or when a woman is concerned who has undergone a laparotomy.

Chronic occlusion is, at a given moment, capable of giving rise to the signs of acute occlusion; but the symptoms are very much less clearly defined; the vomited matters at a later period become fecaloid; the meteorism less considerable.

Upon interrogating the patient, one will recognize anterior disorders, very distinct indications of false obstruction, with a momentary arrest of the fecal matters and sometimes even of the gases. These sypmtoms are met with principally in aged persons, and in the great majority of cases, one finds oneself in presence of a neoplasm of the rectum which the touch will reveal, or of the iliac fossæ which palpation will demonstrate. If it is a question of a narrowing of the small intestine, or of the large intestine, the diagnosis will be impossible; the disorders of digestion, the slow and aggravating course of the affection, will alone afford some suspicion. The contraction of the rectum will always be recognized by the touch.

As to foreign bodies obstructing the calibre of the intestine, biliary calculi alone are capable of producing veritably acute symptoms. Occlusion determined by the presence of a cholelith will be recognized by the fact that it is observed almost exclusively among women who have passed the second half of life. Far from having been preceded by manifestations on the part of the liver, the premonitory or anterior icterus is the more generally absent; but we may discover in the past history of the patient the appearance of pains in the right hypochondrium and quite often a primary attack of vomiting coincident with an obstinate constipation. Finally, a primary biliary calculus may have already been passed in the stools, which permits the diagnosis to be made.

We call attention also to occlusion by fecal masses, which is the type of chronic occlusion, but which, however, has given rise to acute manifestations. One has, in general, to deal with women of very constipated habit, and palpation of the colon in its different parts permits of the determination of a series of rounded masses, echelonned upon the large intestine, of a firm consistence, but sometimes sufficiently soft to be molded under pressure.

Is it possible to diagnosticate the seat of the occlusion?

Many signs have been described as permitting the solution of this important question. Some have rested upon the localization of the pain at the commencement, upon the date of the appearance and the nature of the vomited matters, precocious and non-fecaloid when the obstacle is situated at the commencement of the small intestine; precocious and fecaloid when it resides at the end; tardy and rapidly fecaloid when the large intestine is involved. Observation contradicts these purely theoretical views, and the same may be said about the urine, which is all the more scanty the higher the occlusion is located, by reason of the diminution of the surface of absorption.

Some have thought to succeed by the rectum in overcoming the obstacle by the introduction of sounds; the hand as Simon dangerously advised, or by fluids. It has been claimed that the capacity of the large intestine being two litres, according to the quantity of water which enters it, one could either eliminate the large intestine or ascertain the height at which it was obstructed, but practice has not answered to theory, for a normal intestine may be intolerant and reject the injection.

The form of the abdomen has been invoked by Laugier. The abdomen would project forward in cases of obstruction of the small intestine; flattened and framed in by the dilated large intestine when the obstacle is located at the sigmoid flexure, or at the commencement of the rectum. Unfortunately the meteorism is so great that in all the varieties of occlusion it gives the same form to the belly.

Finally, Bouveret (de Lyon) has drawn attention to the signs of dilatation of the cœcum. This dilatation of the cœcum would indicate an obstruction in the large intestine; but in order to demonstrate it, there must be none of that ever precocious distention of the abdomen which interferes with the manœuvres necessary to recognize it.

As we see it is almost impossible to say where the occlusion is situated, which, however, is a matter of great importance in the selection of the operation (laparotomy or artificial anus).

SELECTIONS.

THE PRESIDENTIAL ADDRESS ON THE INTER-DEPENDENCE OF SCIENCE AND THE HEALING ART.

Delivered before the British Association for the Advancement of Science at Liverpool on Sept. 16, 1896.

By SIR JOSEPH LISTER, BART., M.B., LOND., F.R.C.S. ENG. & EDIN., D.C.L., LL.D., P.B.S.

MY LORD MAYOR, MY LORDS, LADIES, AND GENTLEMEN:—I have first to express my deep sense of gratitude for the great honor conferred upon me by my election to the high office which I occupy to-day. It came upon me as a great surprise. The engrossing claims of surgery have prevented me for many years from attending the meetings of the association, which excludes from her sections medicine in all its branches. This severance of the art of healing from the work of the association was right and indeed inevitable. Not that medicine has little in common with science. The surgeon never performs an operation without the aid of anatomy and physiology; and in what is often the most difficult part of his duty, the selection of the right course to follow, he, like the physician, is guided by pathology, the science of the nature of disease, which, though very difficult from the complexity of its subject matter, has made during the last half century astonishing progress, so that the practice of medicine in every department is becoming more and more based on science as distinguished from empiricism. I propose on the present occasion to bring before you some illustrations of the inter-dependence of science and the healing art.

[Sir Joseph Lister then referred as the first two of his illustrations to the Roentgen rays and anæsthetics, reminding his hearers that as regards the latter agents this was the jubilee year of the discovery of ether. He continued:]

My next illustration may be taken from the work of Pasteur on fermentation. The prevailing opinion regarding this class of phenomena when they first engaged his attention was that they were occasioned primarily by the oxygen of the air acting upon unstable animal or vegetable products, which, breaking up under its influence, communicated disturbance to other organic materials in their vicinity and thus led to their decomposition. Cagniard-Latour had indeed shown several years before that yeast consists essentially of the cells of a microscopic fungus which grows as the sweetwort ferments, and he had attributed the breaking up of the sugar into alcohol and carbonic acid to the growth of the micro-organism. In Germany, Schwann, who independently discovered the yeast plant, had published very striking experiments in support of analogous ideas regarding the putrefaction of meat. Such views had also found other advocates, but they had become utterly discredited, largely through the great authority of Liebig, who bitterly opposed him.

Pasteur, having been appointed as a young man Dean of the Faculty of Sciences in the University of Lille, a town where the products of alcoholic fermentation were staple articles of manufacture, determined to study that process thoroughly, and as a result he became firmly convinced of the correctness of Cagniard-Latour's views regarding it. In the case of other fermentations, however, nothing fairly comparable to the formation of yeast had till then been observed. This was now done by Pasteur for that fermentation in which sugar is resolved into lactic acid. This lactic fermentation was at that time brought about by adding some animal substance, such as fibrin, to a solution of sugar, together with chalk, that should combine with the acid as it was formed. Pasteur saw, what had never before been noticed, that a fine gray deposit was formed, differing little in appearance from the decomposing fibrin, but steadily increasing as the fermentation proceeded. Struck by the analogy presented by the increasing deposit to the growth of yeast in sweetwort he examined it with the microscope and found it to consist of minute particles of uniform size. Pasteur was not a biologist, but although these particles were of extreme minuteness in comparison with the constituents of the yeast plant, he felt convinced that they were of an analogous nature, the cells of a tiny microscopic fungus. This he regarded as the essential ferment, the fibrin or other so-called ferment serving, as he believed, merely the purpose of supplying to the growing plant certain chemical ingredients not contained in the sugar but essential to its nutrition. And the correctness of this view he confirmed in a very striking manner by doing away with the fibrin or other animal material altogether and substituting for it mineral salts containing the requisite chemical elements. A trace of the gray deposit being applied to a solution of sugar containing these salts in addition to the chalk, a brisker lactic fermentation ensued than could be procured in the ordinary way.

I have referred to this research in some detail because it illustrates Pasteur's acuteness as an observer, and his ingenuity in experiment, as well as his almost intuitive perception of truth. A series of other beautiful investigations followed, clearing proving that all true fermentations, including putrefaction, are caused by the growth of micro-organisms. It was natural that Pasteur should desire to know how the microbes which he showed to be the essential causes of the various fermentations took their origin. It was at that period a prevalent notion, even among many eminent naturalists, that such humble and minute beings originated de novo in decomposing organic substances; the doctrine of spontaneous generation, which had been chased successively from various positions which it once occupied among creatures visible to the naked eye, having taken its last refuge where the objects of study were of such minuteness that their habits and history were correspondingly difficult to trace. Here, again, Pasteur at once saw, as if by instinct, on which side the truth lay; and, perceiving its immense importance, he threw himself with ardor into its demonstration. I may describe briefly one class of experiments which he performed with this object. He charged a series of narrow-necked glass flasks with a decoction of yeast, a liquid peculiarly liable to alteration on exposure to the air. Having boiled the liquid in each flask to kill any living germs it might contain, he sealed its neck with a blow-pipe during ebullition, after which, the flask being allowed to cool, the steam within it condensed, leaving a vacuum above the liquid. If, then, the neck of the flask were broken in any locality, the air at that particular place would rush in to fill the vacuum, carrying with it any living microbes that might be floating in it. The neck of the flask having been again sealed, any germs so introduced would in due time manifest their presence by developing in the clear liquid. When any of such a series of flasks were opened and resealed in an inhabited room, or under the trees of a forest, multitudes of minute living forms made their appearance in them; but if this was done in a cellar long unused, where the suspended organisms, like other dust, might be expected to have all fallen to the ground, the decoction remained perfectly clear and unaltered. The oxygen and other gaseous constituents of the atmosphere were thus shown to be of themselves incapable of inducing any organic development in yeast water. Such is a sample of the many well-devised experiments by which he carried to most minds the conviction that, as he expressed it, "la génération spontanée est une chimère," and that the humblest and minutest living organisms can only originate by parentage from beings like themselves. Pasteur pointed out the enormous importance of these humble organisms in the economy of nature. It is by their agency that the dead bodies of plants and animals are resolved into simpler compounds fitted for assimilation by new living forms. Without their aid the world would be, as Pasteur expresses it, encombré de cadavres. They are essential, not only to our well-being, but to our very existence. Similar microbes must have discharged the same necessary function of removing refuse, and providing food for successive generations of plants and animals during the past periods of the world's history; and it is interesting to think that organisms as simple as can well be conceived to have existed when life first appeared upon our globe have in all probability propagated the same lowly, but most useful offspring during the ages of geological time. Pasteur's labors on fermentation have had a very important influence upon surgery. I have been often asked to speak on my share in this matter before a public audience; but I have hitherto refused to do so, partly because the details are so entirely technical, but chiefly because I have felt an invincible repugnance to what might seem to savor of self-advertisement. The latter objection now no longer exists, since advancing years have indicated that it is right for me to leave to younger men the practice of my dearly loved profession. And it will perhaps be expected that, if I can make myself intelligible, I should say something upon the subject on the present occasion.

[The president then gave a short account of the behavior of wounds in the pre-antiseptic days and continued:]

It will be readily understood that the inflammation which so often frustrated the surgeon's endeavors after primary union was, in my opinion, essentially due to decomposition of blood within the wound. These and many other considerations had long impressed me with the greatness of the evil of putrefaction in surgery. I had done my best to mitigate it by scrupulous ordinary cleanliness and the use of various deodorant lotions. But to prevent it altogether appeared hopeless, while we believed with Liebig that its primary cause was the atmospheric oxygen, which in accordance with the researches of Graham, could not fail to be perpetually diffused through the porous dressings which were used to absorb the blood discharged from the wound. But when Pasteur had shown that putrefaction was a fermentation caused by the growth of microbes, and that these could not arise de novo in the decomposable substance, the problem assumed a more hopeful aspect. If the wound could be treated with some substance which, without doing too serious mischief to the human tissues, would kill the microbes already contained in it, and prevent the future access of others in the living state, putrefaction might be prevented, however freely the air with its oxygen might enter. I had heard of carbolic acid as having a remarkable deodorizing effect upon sewage, and having obtained from my colleague, Dr. Anderson, Professor of Chemistry in the University of Glasgow, a sample which he had of this product, then little more than a chemical curiosity in Scotland, I determined to try it in compound fractures. Applying it undiluted to the wound, with an arrangement for its occasional renewal, I had the joy of seeing these formidable injuries follow the same safe and tranquil course as simple fractures, in which the skin remains unbroken. At the same time we had the intense interest of observing in open wounds what had previously been hidden from human view—the manner in which subcutaneous injuries are repaired. Of special interest was the process by which portions of tissue killed by the violence of the accidents were disposed of, as contrasted with what had till then been invariably witnessed. Dead parts had been always seen to be gradually separated from the living by an inflammatory process and thrown off as sloughs. But when protected by the antiseptic dressing from becoming putrid, and therefore irritating, a structure deprived of its life caused no disturbance in its vicinity; and, on the contrary, being of a nutritious nature, it served as pabulum for the growing elements of the neighboring living structures, and these became in due time entirely substituted for it. Even dead bone was seen to be thus replaced by living osseous tissue. This suggested the idea of using threads of dead animal tissue for tying bloodvessels; and this was realized by means of catgut which is made from the intestine of the sheep. If deprived of living microbes and otherwise properly prepared, catgut answers its purpose completely, the knot holding securely, while the ligature around the vessel becomes gradually absorbed and replaced by a ring of living tissue. The threads, instead of being left long as before, could now be cut short, and the tedious process of separation of the ligature with its attendant serious danger of bleeding was avoided.

[Sir Joseph Lister then referred to the early technique of the antiseptic method. Proceeding, he said:]

Nine years later, however, at the Berlin Congress in 1890, I was able to bring forward what was, I believe, absolute demonstration of the harmlessness of the atmospheric dust in surgical operations. This conclusion has been justified by subsequent experience; the irritation of the wound by antiseptic irrigation and washing may therefore now be avoided and nature left quite undisturbed to carry out her best methods of repair, while the surgeon may conduct his operations as simply as in former days, provided always that, deeply impressed with the tremendous importance of his object, and inspiring the same conviction in all his assistants, he vigilantly maintains from first to last, with a care that once learnt becomes instinctive, but for the want of which nothing else can compensate, the use of the simple means which will suffice to exclude from the wound the coarser forms of septic impurity. Even our earlier and ruder methods of carrying out the antiseptic principle soon produced a wonderful change in my surgical wards in the Glasgow Royal Infirmary, which, from being some of the most unhealthy in the Kingdom, became, as I believe I may say without exaggeration, the healthiest in the world; while other wards, separated from mine only by a passage a few feet broad, where former modes of treatment were for a while continued, retained their former insalubrity. This result, I need hardly remark, was not in any degree due to special skill on my part, but simply to the strenuous endeavor to carry out strictly what seemed to me a principle of supreme importance. Equally striking changes were afterward witnessed in other institutions. Of these I may give one example. In the great Allgemeines Krankenhaus of Munich, hospital gangrene had become more and more rife from year to year, till at length the frightful condition was reached that 80 per cent. of all wounds became affected by it. It is only just to the memory of Professor von Nussbaum, then the head of that establishment, to say that he had done his utmost to check this frightful scourge; and that the evil was not caused by anything peculiar in his management was shown by the fact that in a private hospital under his care there was no unusual unhealthiness. The larger institution seemed to have become hopelessly infected, and the city authorities were contemplating its demolition and reconstruction. Under these circumstances, Professor von Nussbaum despatched his chief assistant, Dr. Lindpaintner, to Edinburgh, where I at the time occupied the chair of clinical surgery, to learn the details of the antiseptic system as we then practiced it. He remained until he had entirely mastered them, and after his return all the cases were on a certain day dressed on our plan. From that day forward not a single case of hospital gangrene occurred in the Krankenhaus. The fearful disease, pyæmia, likewise disappeared, and erysipelas soon followed its example. But it was by no means only in removing the unhealthiness of hospitals that the antiseptic system showed its benefits. Inflammation being suppressed with attendant pain, fever, and wasting discharge, the sufferings of the patient were, of course, immensely lessened; rapid primary union being now the rule, convalescence was correspondingly curtailed; while as regards safety and the essential nature of the mode of repair it became a matter of indifference whether the wound had clean-cut surfaces which could be closely approximated, or whether the injury inflicted had been such as to cause destruction of tissue. And operations which had been regarded from time immemorial as unjustifiable were adopted with complete safety. It pleases me to think that there is an ever-increasing number of practitioners throughout the world to whom this will not appear the language of exaggeration. There are cases in which, from the situation of the part concerned or other unusual circumstances, it is impossible to carry out the antiseptic system completely. These, however, are quite exceptional; and even in them much has been done to mitigate the evil which cannot be altogether avoided. I ask your indulgence if I have seemed to dwell too long upon matters in which I have been personally concerned. I now gladly return to the labors of others.

The striking results of the application of the germ theory to surgery acted as a powerful stimulus to the investigation of the micro-organisms concerned; and it soon appeared that putrefaction was by no means the only evil of microbic origin to which wounds were liable. I had myself very early noticed that hospital gangrene was not necessarily attended by any unpleasant odor; and I afterwards made a similar observation regarding the matter formed in a remarkable epidemic of erysipelas in Edinburgh, obviously of infective character. I had also seen a careless dressing followed by the occurrence of suppuration without putrefaction. And as these non-putrefactive disorders had the same self-propagating property as ferments, and were suppressed by the same antiseptic agencies which were used for combating the putrefactive microbes, I did not doubt that they were of an analogous origin; and I ventured to express the view that, just as the various fermentations had each its special microbe, so it might be with the various complications of wounds. This surmise was afterwards amply verified. Professor Ogston of Aberdeen was an early worker in this field, and showed that in acute abscesses—that is to say, those which run a rapid course—the matter, although often quite free from unpleasant odor, invariably contains micro-organisms belonging to the group which, from the spherical form of their elements, are termed micrococci; and these he classed as streptococci or staphylococci according as they were arranged in chains or disposed in irregular clusters like bunches of grapes. The German pathologist, Fehleisen, followed with a beautiful research, by which he clearly proved that erysipelas is caused by a streptococcus. A host of earnest workers in different countries have cultivated the new science of bacteriology and while opening up a wide fresh domain of biology have demonstrated in so many cases the casual relation between special micro-organisms and special diseases, not only in wounds but in the system generally, as to afford ample confirmation of the induction which had be enmade by Pasteur, that all infective disorders are of microbe origin. Not that we can look forward with anything like confidence to being able ever to see the materies morbi of every disease of this nature. One of the latest of such discoveries has been that by Pfeiffer of Berlin of the bacillus of influenza, perhaps the most minute of all micro-organisms ever yet detected. The bacillus of anthrax, the cause of a plague among cattle in some parts of Europe, and often communicated to sorters of foreign wool in this country, is a giant as compared with this tiny being; and supposing the microbe of any infectious fever to be as much smaller than the influenza bacillus as this is less than that of anthrax, a by no means unlikely hypothesis, it is probable that it would never be visible to man. The improvements of the microscope, based on the principle established by my father in the earlier part of the century, have apparently nearly reached the limits of what is possible. But that such parasites are really the causes of all this great class of diseases can no longer be doubted.

The first rational step towards the prevention or cure of disease is to know its cause; and it is impossible to over-estimate the practical value of researches such as those to which I am now referring. Among their many achievements is what may be fairly regarded as the most important discovery ever made in pathology, because it revealed the true nature of the disease which causes more sickness and death of the human race than any other. It was made by Robert Koch, who greatly distinguished himself when a practitioner in an obscure town in Germany by the remarkable combination of experimental acuteness and skill, chemical and optical knowledge, and successful micro-photography, which he brought to bear upon the illustration of infective diseases of wounds in the lower animals; in recognition of which service the enlightened Prussian Government at once appointed him to an official position of great importance in Berlin. There he conducted various important researches; and at the London Congress in 1881 he showed to us for the first time the bacillus of tubercle. Wonderful light was thrown by this discovery upon a great group of diseases which had before been rather guessed than known to be of an allied nature; a precision and efficacy never before possible was introduced into their surgical treatment, while the physician became guided by new and sure light as regards their diagnosis and prevention. At that same London Congress Koch demonstrated to us his "plate culture" of bacteria, which was so important that I must devote a few words to its description. With a view to the successful study of the habits and effects of any particular microbe outside the living body it is essential that it should be present unmixed in the medium in which it is cultivated. It can be readily understood how difficult it must have been to isolate any particular micro-organism when it existed mixed, as was often the case, with a multitude of other forms. In fact, the various ingenious attempts made to effect this object had often proved entire failures. Koch, however, by an ingenious procedure converted what had been before impossible into a matter of the utmost facility. In the broth or other nutrient liquid which was to serve as food for the growing microbe, he dissolved, by aid of heat, just enough gelatin to insure that, while it should become a solid mass when cold, it should remain fluid though reduced in temperature so much as to be incapable of killing living germs. To the medium thus partially cooled was added some liquid containing, among others, the microbe to be investigated; and the mixture was thoroughly shaken so as to diffuse the bacteria and separate them from each other. Some of the liquid was then poured out in a thin layer upon a glass plate and allowed to cool so as to assume the solid form. The various microbes, fixed in the gelatin and so prevented from intermingling, proceeded to develop each its special progeny, which in course of time showed itself as an opaque speck in the transparent film. Any one of such specks could now be removed and transferred to another vessel in which the microbe composing it grew in perfect isolation. Pasteur was present at this demonstration and expressed his sense of the great progress effected by the new method. It was soon introduced into his own institute and other laboratories throughout the world, and it has immensely facilitated bacteriological study. One fruit of it in Koch's own hands was the discovery of the microbe of cholera in India, whither he went to study the disease. This organism was termed by Koch from its curved form the "comma bacillus," and by the French the "cholera vibrio." Great doubts were for a long time felt regarding this discovery. Several other kinds of bacteria were found of the same shape, some of them producing very similar appearances in culture media. But bacteriologists are now universally agreed that, although various other conditions are necessary to the production of an attack of cholera besides the mere presence of the vibrio, yet it is the essential *materies morbi*, and it is by the aid of the diagnosis which its presence in any case of true cholera enables the bacteriologist to make that threatened invasions of this awful disease have of late years been so successfully repelled from our shores. If bacteriology had done nothin more for us than this it might well have earned our gratitude.

I have next to invite your attention to some earlier work of Pasteur. There is a disease known in France under the name of *cholera des poules*, which often produced great havoc among the poultry yards of Paris. It had been observed that the blood of birds that had died from this disease was peopled by a multitude of minute bacteria not very dissimilar in form and size to the microbe of the lactic ferment to which I have before referred. And Pasteur found that if this bacterium were cultivated outside the body for a protracted period under certain conditions it underwent a remarkable diminution of its virulence; so that, if inoculated into a healthy fowl, it no longer caused the death of the bird, as it would have done in its original condition, but produced a milder form of the disease which was not fatal. And the altered character of the microbe, caused by certain conditions, was found to persist in successive generations cultivated in the ordinary way. Thus was discovered the great fact of what Pasteur termed the *atténuation des virus*, which at once gave the clue to understanding what had before been quite mysterious—the difference in virulence of the same disease in different epidemics. But he made the further very important observation that a bird which had gone through the mild form of the complaint had acquired immunity against it in its most virulent condition. Pasteur afterward succeeded in obtaining mitigated varieties of microbes for some other diseases, and he applied with great success the principle which he had discovered in fowl-cholera for protecting the larger domestic animals against the plague of anthrax. The preparations used for such preventive inoculations he termed "vaccins" in honor of our great countryman, Edward Jenner. For Pasteur at once saw the analogy between the immunity to fowl-cholera produced by its attenuated virus and the protection afforded against smallpox by vaccination. And while pathologists still hesitated he had no doubt of the correctness of Jenner's expression *variolæ vaccinæ* , or smallpox in the cow. It is just a hundred years since Jenner made the crucial experiment of inoculating with smallpox a boy whom he had previously vaccinated, the result being, as he anticipated, that the boy was quite unaffected. It may be remarked that this was a perfectly legitimate experiment, involving no danger to the subject of it. Inoculation was at that time the established practice; and if vaccination should prove nugatory, the inoculation would be only what would have been otherwise called for, while it would be perfectly harmless if the hoped-for effect of vaccination had been produced. We are practical people, not much addicted to personal commemorations, although our nation did indeed celebrate with fitting splendor the jubilee of the reign of our beloved Queen, and at the invitation of Glasgow, the scientific world has lately marked in a manner, though different, as imposing, the jubilee of the life-work of a sovereign in science (Lord Kelvin). But while we cannot be astonished that the centenary of Jenner's immortal discovery should have failed to receive general recognition in this country, it is melancholy to think that this year should in his native county have been distinguished by a terrible illustration of the results which would sooner or later inevitably follow the general neglect of his prescriptions. I have no desire to speak severely of the Gloucester guardians. They are not sanitary authorities and had not the technical knowledge necessary to enable them to judge between the teachings of true science and the declamations of misguided, though well-meaning, enthusiasts. They did what they believed to be right; and when roused to a sense of the greatness of their mistake they did their very best to repair it, so that their city is said to be now the best vaccinated in Her Majesty's dominions. But though by their praiseworthy exertions they succeeded in promptly checking the raging epidemic, they cannot recall the dead to life, or restore beauty to marred features, or sight to the blinded eyes. Would that the entire country and our legislature might take duly to heart this object lesson. How completely the medical profession were convinced of the efficacy of vaccination in the early part of this century was strikingly illustrated by an account given by Professor Crookshank in his interesting history of this subject of several prominent medical men in Edinburgh meeting to see the, to them unprecedented, fact of a vaccinated person having taken the smallpox. It has, of course, since become well known that the milder form of the disease, as modified by passing through the cow, confers a less permanent protection than the original human disorder. This it was, of course, impossible for Jenner to foresee. It is, indeed, a question of degree, since a second attack of ordinary smallpox is occasionally known to occur, and vaccination, long after it has ceased to give perfect immunity, greatly modifies the character of the disorder and diminishes its danger. And, happily, in re-vaccination after a certain number of years we have the means of making Jenner's work complete. I understand that the majority of the commissioners, who have recently issued their report upon this subject, while recognizing the value and importance of re-vaccination, are so impressed with the difficulties that would attend making it compulsory by legislation that they do not recommend that course; although it is advocated by two of their number who are of peculiarly high authority on such a question. I was lately told by a Berlin professor that no serious difficulty is experienced in carrying out the compulsory law that prevails in Germany. The masters of the schools are directed to ascertain in the case of every child attaining the age of twelve years whether re-vaccination has been practised. If not, and the parents refuse to have it done, they are fined one mark. If this does not prove effectual the fine is doubled; and if even the double penalty should not prove efficacious, a second doubling of it would follow, but, as my informant remarked, it is very seldom that it is called for. The result is that smallpox is a matter of extreme rarity in that country, while it is absolutely unknown in the huge German army, in consequence of the rule that every soldier is re-vaccinated on entering the service. Whatever view our legislature may take on this question, one thing seems to me clear; that it will be the duty of the government to encourage by every available means the use of calf lymph, so as to exclude the possibility of the communication of any human disease to the child, and to institute such efficient inspection of vaccination institutes as shall insure careful antiseptic arrangements, and so prevent contamination by extraneous microbes. If this were done, "conscientious objections" would cease to have any rational basis. At the same time, the administration of the regulations on vaccination should be transferred (as advised by the commissioners) to competent sanitary authorities.

But to return to Pasteur. In 1880 he entered upon the study of that terrible, but then most obscure disease, hydrophobia or rabies, which from its infective character he was sure was of microbic origin, although no micro-organism could be detected in it. He early demonstrated the new pathological fact that the virus had its essential seat in the nervous system. This proved the key to his success in this subject. One result that flowed from it has been the cause of unspeakable consolation to many. The foolish practice is still too prevalent of killing the dog that has bitten any one, on the absurd notion that if it were mad its destruction would prevent the occurrence of hydrophobia in the person bitten. The idea of the bare possibility of the animal having been so affected causes an agony of suspense during the long weeks or months of possible incubation of the disease. Very serious nervous symptoms aping true hydrophobia have been known to result from the terror thus inspired. Pasteur showed that if a little of the brain or spinal cord of a dog that had been really mad was inoculated in an appropriate manner into a rabbit it infallibly caused rabies in that animal in a few days. If, therefore, such an experiment was made with a negative result, the conclusion might be drawn with certainty that the dog had been healthy. It is perhaps right that I should say that the inoculation is painlessly done under an anæsthetic, and that in the rabbit rabies does not assume the violent form that it does in the dog, but produces gradual loss of power with little, if any, suffering. This is more satisfactory because rabbits in which the disease has been thus artificially induced are employed in carrying out what was Pasteur's greatest triumph, the preventive treatment of hydrophobia in the human subject. We have seen that Pasteur discovered that microbes might under some circumstances undergo mitigation of their virulence. He afterwards found that under different conditions they might have it exalted, or, as he expressed it, there might be a *renforcement du virus*. Such proved to be the case with rabies in the rabbit, so that the spinal cords of animals which had died from it contained the poison in a highly intensified condition. But he also found that if such a highly virulent cord was suspended under strict antiseptic precautions in a dry atmosphere at a certain temperature, it gradually from day to day lost in potency, till in course of time it became absolutely inert. If now an emulsion of such a harmless cord was introduced under the skin of an animal, as in the subcutaneous administration of morphia, it might be followed without harm another day by a similar dose of a cord still rather poisonous; and so from day to day stronger and stronger injections might be used, the system becoming gradually accustomed to the poison, till a degree of virulence had been reached far exceeding that of the bite of a mad dog. When this had been attained, the animal proved incapable of taking the disease in the ordinary way; and more than that, if such treatment was adopted after an animal had already received the poison, provided that too long a time had not elapsed, the outbreak of the disease was prevented. It was only after great searching of heart that Pasteur after consultation with some trusted medical friends, ventured upon trying this practice upon a man. It has since been extensively adopted in various parts of the world with increasing success as the details of the method were improved. It is not, of course, the case that every one bitten by a really rabid animal takes the disease, but the percentage of those who do so, which was formerly large, has been reduced almost to zero by this treatment, if not too long delayed. While the intensity of rabies in the rabbit is undoubtedly due to a peculiarly virulent form of the microbe concerned, we cannot suppose that the daily diminishing potency of the cord suspended in dry, warm air is an instance of attenuation of virus, using the term "virus" as synonymous with the microbe concerned. In other words, we have no reason to believe that the special micro-organism of hydrophobia continues to develop in the dead cord and produce successively a milder and milder progeny, since rabies cannot be cultivated in the nervous system of a dead animal. We must rather conclude that there must be some chemical poison present which gradually loses its potency as time passes; and this leads me to refer to another most important branch of this large subject of bacteriology—that of the poisonous products of microbes.

It was shown several years ago by Roux and Yersin, working in the Institut Pasteur, that the crust or false membrane which forms upon the throats of patients affected with diphtheria contains bacteria which can be cultivated outside the body in a nutrient liquid, with the result that it acquires poisonous qualities of astonishing intensity, comparable to that of the secretion of the poison-glands of the most venomous serpents. And they also ascertained that the liquid retained this property after the microbes had been removed from it by filtration, which proved that the poison must be a chemical solution, as distinguished from the living element which had produced it. These poisonous products of bacteria, or toxins as they have been termed, explain the deadly effects of some microbes, which it would otherwise be impossible to understand. Thus, in diphtheria itself the special bacillus which was shown by Löffler to be its cause, does not become propagated in the blood, like the microbe of chicken cholera, but remains confined to the surface on which it first appeared; but the toxin which it secretes is absorbed from that surface into the blood and so poisons the system. Similar observations have been made with regard to the microbes of some other diseases, as, for example, the bacillus of tetanus or lockjaw. This remains localized in the wound, but forms a special toxin of extreme potency, which becomes absorbed and diffused through the body. Wonderful as it seems, each poisonous microbe appears to form its own peculiar toxin. Koch's tuberculin was of this nature, a product of the growth of the tubercle bacillus in culture media. Here, again, great effects were produced by extremely minute quantities of the substance, but here a new peculiarity showed itself—viz., that patients affected with tuberculous disease in any of its varied forms exhibited inflammation in the affected part and general fever after receiving under the skin an amount of the material which had no effect whatever upon healthy persons. I witnessed in Berlin some instances of these effects, which were simply astounding. Patients affected with a peculiar form of obstinate ulcer of the face showed, after a single injection of the tuberculin, violent inflammatory redness and swelling of the sore and surrounding skin; and, what was equally surprising, when this disturbance subsided the disease was found to have undergone great improvement. By repetitions of such procedures ulcers which had previously been steadily advancing, in spite of ordinary treatment, became greatly reduced in size and in some instances apparently cured. Such results led Koch to believe that he had obtained an effectual means of dealing with the tuberculous disease in all its forms. Unhappily the apparent cure proved to be only of transient duration and the high hopes which had been inspired by Koch's great reputation were dashed. It is but fair to say that he was strongly urged to publish before he was himself disposed to do so, and we cannot but regret that he yielded to the pressure put upon him. But though Koch's sanguine anticipations were not realized, it would be a great mistake to suppose that his labors with tuberculin have been fruitless. Cattle are liable to tubercle, and when affected with it may become a very serious source of infection for human beings, more especially when the disease affects the udders of cows, and so contaminates the milk. By virtue of the close affinity that prevails between the lower animals and ourselves, in disease as well as in health, tuberculin produces fever in tuberculous cows in doses which do not affect healthy beasts. Thus, by the subcutaneous use of a little of the fluid, tubercle latent in internal organs of an apparently healthy cow can be with certainty revealed, and the slaughter of the animal after this discovery protects man from infection.

It has been ascertained that glanders present a precise analogy with tubercule as regards the effects of its toxic products. If the microbe which has been found to be the cause of this disease is cultivated in appropriate media, it produces a poison which has received the name of mallein, and the subcutaneous injection of a suitable dose of this fluid into a glandered horse causes striking febrile symptoms which do not occur in a healthy animal. Glanders, like tubercle, may exist in insidious latent forms which there was formerly no means of detecting, but which are at once disclosed by this means. If a glandered horse has been accidentally introduced into a large stable, this method of diagnosis surely tells if it has infected others. All receive a little mallein. Those which have become affected with fever are slaughtered, and thus not only is the disease prevented from spreading to other horses, but the grooms are protected from a mortal disorder. This valuable resource sprang from Koch's work on tuberculin, which has also indirectly done good in other ways. His distinguished pupil, Behring, has expressly attributed to those researches the inspiration of the work which led him and his since famous collaborator, the Japanese Kitasato, to their surprising discovery of antitoxic serum. They found that if an animal of a species liable to diphtheria or tetanus received a quantity of the respective toxin, so small as to be harmless, and afterwards, at suitable intervals, successively stronger and stronger doses, the creature in course of time acquired such a tolerance for the poison as to be able to receive with impunity a quantity very much greater than would at the outset have proved fatal. So far we have nothing more than seems to correspond with the effects of the increasingly potent cords in Pasteur's treatment of rabies. But what was entirely new in their results was that, if blood was drawn from an animal which had acquired this high degree of artificial immunity and some of the clear fluid or serum which exuded from it after it had clotted was introduced under the skin of another animal, this second animal acquired a strong, though more transient immunity against the particular toxin concerned. The serum in some way counteracted the toxin or was antitoxic. But, more than that, if some of the antitoxic serum was applied to an animal after it had already received a poisonous dose of the toxin, it preserved the life of the creature, provided that too long a time had not elapsed after the poison was introduced. In other words, the antitoxin proved to be not only preventive, but curative. Similar results were afterwards obtained by Ehrlich, of Berlin, with some poisons not of bacterial origin, but derived from the vegetable kingdom; and quite recently the independent labors of Calmette, of Lille, and Fraser, of Edinburgh, have shown that antidotes of wonderful efficacy against the venom of serpents may be procured on the same principle. Calmette has obtained antitoxin so powerful that a quantity of it—only a 200,000th part of the weight of an animal—will protect it perfectly against a dose of the secretion of the poison glands of the most venomous serpents known to exist, which, without such protection, would have proved fatal in four hours. For curative purposes larger quantities of the remedy are required, but cases have already been published by Chalmette in which death appears to have been averted in the human subject by this treatment. Behring's darling object was to discover means of curing tetanus and diphtheria in man. In tetanus the conditions are not favorable, because the specific bacilli lurk in the depths of the wound and only declare their presence by symptoms caused by their toxin having been already in a greater or less amount diffused through the system; and in every case of this disease there must be a fear that the antidote may be applied too late to be useful. But in diphtheria the bacilli very early manifest their presence by the false membrane which they cause upon the throat, so that the antitoxin has a fair chance; and here we are justified in saying that Behring's object has been attained. The problem, however, was by no means so simple as in the case of some chemical poison. However effectual the antitoxin might be against the toxin, if it left the bacilli intact not only would repeated injections be required to maintain the transient immunity to the poison perpetually secreted by the microbes, but the bacilli might by their growth and extension cause obstruction of the respiratory passages. Roux, however, whose name must always be mentioned with honor in relation to this subject, effectually disposed of this difficulty. He showed by experiments on animals that a diphtheritic false membrane, rapidly extending and accompanied by surrounding inflammation, was brought to a stand by the use of the antitoxin and soon dropped off, leaving a healthy surface. Whatever be the explanation, the fact was thus established that the antitoxic serum, while it renders the toxin harmless, causes the microbe to languish and disappear. No theoretical objection could now be urged against the treatment; and it has during the last two years been extensively tested in practice in various parts of the world, and it has gradually made its way more and more into the confidence of the profession. One important piece of evidence in its favor in this country is derived from the report of the six large hospitals under the management of the London Asylums Board. The medical officers of these hospitals at first naturally regarded the practice with scepticism; but as it appeared to be at least harmless, they gave it a trial, and during the year 1895 it was very generally employed upon the 2,182 cases admitted; and they have all become convinced of its great value. In the nature of things, if the theory of the treatment is correct, the best results must be obtained when the patients are admitted at an early stage of the attack, before there has been time for much poisoning of the system; and accordingly we learn from the report that, comparing 1895 with 1894, during which latter year the ordinary treatment had been used, the percentage of mortality in all the six hospitals combined, among the patients admitted on the first day of the disease, which in 1894 was 22.5, was only 4.6 in 1895; while for those admitted on the second day the numbers are 27 for 1894 and 14.8 for 1895. Thus for cases admitted on the first day the mortality was only one-fifth of what it was in the previous year, and for those entering on the second it was halved. Unfortunately in the low parts of London, which furnish most of these patients, the parents too often delay sending in the children till much later; so that on an average no less than 67.5 per cent. were admitted on the fourth day of the disease or later. Hence, the aggregate statistics of all cases are not nearly so striking. Nevertheless, taking it altogether, the mortality in 1895 was less than had ever before been experienced in those hospitals. I should add that there was no reason to think that the disease was of a milder type than usual in 1895; and no change whatever was made in the treatment except as regards the antitoxic injections.

There is one piece of evidence recorded in the report which, though it is not concerned with high numbers, is well worthy of notice. It relates to a special institution to which convalescents from scarlet fever are sent from all the six hospitals. Such patients occasionally contract diphtheria, and when they do so the added disease has generally proved extremely fatal. In the five years preceding the introduction of the treatment with antitoxin the mortality from this cause had never been less than 50 per cent., and averaged on the whole 61.9 per cent. During 1895, under antitoxin, the deaths among the 119 patients of this class were only 7.5 per cent., or one-eighth of what had been previously experienced. This very striking result seems to be naturally explained by the fact that these patients being already in the hospital when the diphtheria appeared an unusually early opportunity was afforded for dealing with it. There are certain cases of so malignant a character from the first that no treatment will probably ever be able to cope with them. But taking all cases together, it seems probable that Behring's hope that the mortality may be reduced to 5 per cent. will be fully realized when the public become alive to the paramount importance of having the treatment commenced at the outset of the disease. There are many able workers in the field of bacteriology whose names time does not permit me to mention and to whose important labors I cannot refer; and even those researches of which I have spoken have been of course, most inadequately dealt with. I feel this especially with regard to Pasteur, whose work shines out more brightly the more his writings are perused.

I have, lastly, to bring before you a subject which, though not bacteriological, has intimate relations with bacteria.

[Sir Joseph Lister then described Metchnikoff's observations on phagocytosis. He concluded:]

I was myself peculiarly interested by these observations of Metchnikoff's because they seemed to me to afford clear explanation of the healing of wounds by first intention under circumstances before incomprehensible. This primary union was sometimes seen to take place in wounds treated with water-dressing; that is to say, a piece of wet lint covered with a layer of oiled silk to keep it moist. This, though cleanly when applied, was invariably putrid within twenty-four hours. This layer of blood between the cut surfaces was thus exposed at the outlet of the wound to a most potent septic focus. How was it prevented from putrefying, as it would have done under such influence if, instead of being between divided living tissues, it had been between plates of glass or other indifferent material? Pasteur's observations pushed the question a step further. It now was, How were the bacteria of putrefaction kept from propagating in the decomposable film? Metchnikoff's phagocytosis supplied the answer. The blood between the lips of the wound become rapidly peopled with phagocytes, which kept guard against the putrefactive microbes and seized them as they endeavored to enter. If phagocytosis was ever able to cope with septic microbes in so concentrated and intense a form it could hardly fail to deal effectually with them in the very mitigated condition in which they are present in the air. We are thus strongly confirmed in our conclusion that the atmospheric dust may safely be disregarded in our operations; and Metchnikoff's researches, while they have illumined the whole pathology of infective diseases, have beautifully completed the theory of antiseptic treatment in surgery. I might have taken equally striking illustrations of my theme from other departments in which microbes play no part. In fact, any attempt to speak of all that the art of healing has borrowed from science and contributed to it during the past half-century would involve a very extensive dissertation on pathology and therapeutics. I have culled specimens from a wide field; and I only hope that in bringing them before you I have not overstepped the bounds of what is fitting before a mixed company. For many of you my remarks can have had little if any novelty; for others they may perhaps possess some interest as showing that medicine is no unworthy ally of the British Association—that, while her practice is ever more and more based on science the ceaseless efforts of her votaries to improve what have been fittingly designed *quae prosunt omnibus artes,* are ever adding largely to the sum of abstract knowledge.

THE TREATMENT OF APPENDICITIS.[1]

By CHARLES McBURNEY, M.D., of New York,

Visiting Surgeon to Roosevelt Hospital, Professor of Clinical Surgery in the College of Physicians and Surgeons, New York.

I intend to give you an informal talk to-night dwelling especially upon certain questions of treatment of appendicitis. It is a singular fact that inflammation of the appendix vermiformis as a separate disease had not been identified nor described by pathologists previous to 1886. In that year a paper upon pericecitis appeared by Dr. Reginald H. Fitz of Boston. This gave the true pathology of a disease which is now so well known. In the development of our present knowledge of the pathology and treatment of appendicitis there have been three well-marked stages. The first stage began when Willard Parker first practised opening the so-called perityphlitic abscesses. This was in 1866 or 1867. Dr. Henry B. Sands was at that time Dr. Parker's partner, and I was a student in Dr. Parker's office. Dr. Parker deserved great credit for his boldness and originality. At first he opened only large, dull, and clearly defined abscesses, letting the smaller and less plainly marked ones go. After Dr. Parker's retirement from active practice, Dr. Sands carried on the work which the former had so well begun. As said above, however, the true pathology of the condition was not known until the appearance of Dr. Fitz's paper in 1886, which explained the lesion so accurately that no material addition has since been made to the pathogenesis of the disease. The proper surgical treatment was not determined for some time after this. This period of the development of the treatment was the second stage. Dr. Sands was constantly working at this surgical problem, and to him is due the whole credit for bringing the operation to a state of perfection. Thus the third stage of the development of the surgical treatment of appendicitis was reached. Dr. Sands first successfully removed the diseased appendix from a young boy, a patient of Dr. Simon Baruch. It is a matter of surprise that the name of this operator is so seldom met with in the literature of the subject at the present day. Certainly one would expect that he would receive more general acknowledgment for this brilliant surgical achievement.

The obstinate resistance which Dr. Sands' operation called forth in many quarters seems laughable as we look at it now. In that day nearly every one agreed that the disease, in common with general peritonitis, should be treated with opium. Even progressive surgeons shivered at the thought of an operation which proposed to invade the abdominal cavity so freely. Only six years ago a surgeon of large experience decided against operation in a case of appendicitis, because, as he alleged, all of his cases had recovered without operation. Another surgeon, in a paper published five years ago, claimed that twenty other diseases might be confounded with appendicitis. It was alleged in a paper that appeared in a recent number of a prominent medical journal that the cause of the disease is rheumatism, and the treatment should therefore be anti-rheumatic. Such suggestions may be, and no doubt have been, productive of much harm by inducing practitioners to dally with cases of appendicitis expecting the administration of salicylates, salol, etc., to do good. Is there any purely medical measure which may be relied on to cure this disease? I know of none. Perhaps it is as well that we should know that there is no remedy of a medical nature, because while we are searching for some drug to cure the patient we may be doing him irreparable damage by delay. Appendicitis must logically, I think, be placed among the surgical diseases. This does not mean of course that every case must be operated upon, nor, on the other hand, does it follow that because certain cases recover without surgical interference the complaint is to be taken out of the list of surgical diseases. I would not be understood as saying that many purely medical men are not as clever as any surgeon in making the diagnosis of appendicitis, and as anxious to have the case treated on the best surgical principles. There are two classes of medical men who take different views of the treatment of the disease: one I have just mentioned; the other looks upon all cases as really medical in character, seldom requiring any surgical measures for their relief. These practitioners approach the subject from totally different standpoints, and too often prejudge a case without studying the conditions with sufficient care. The disease is therefore not so well studied or handled as it might be. This state of things is, however, gradually being cleared up, and in time the disease will surely be placed in its proper category.

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[1] Read before the Orange Mountain Medical Society.

As illustrating the evils of an improperly handled case, I might cite one which has recently occurred to me, and which was one of the worst cases I have seen. A young lady of good family, under the care of a medical man, who, I am glad to say, was not a regular practitioner, was suffering from her ninth attack of appendicitis, which she and her family were assured was enterocolitis. The patient herself recognized the nature of her ailment, and demanded an operation. I was called in after the patient had been ill eight days, and while recognizing the extreme gravity of the situation yielded to the solicitations of the patient and her friends to give her the only chance which, in my judgment remained, viz., an operation. The first incision gave vent to a quart of pus, which spurted to the height of a foot. This was an aggravated case of septic peritonitis due to appendicitis, and naturally terminated fatally.

We are very far from possessing any exact knowledge as to the primary cause of appendicitis. My whole thought has run toward a stoppage of the drainage from the appendix into the colon as the true cause of the disease. There may be an interference with the emptying of the contents of this little tube, due to several lesions: as, for instance, a concretion in the appendix itself, or a stricture, or accumulations of fat causing displacements, or there may be a kink or twist, either in the appendix itself, or in a fold of the colon. I have never seen a case in which there was no interference with the drainage, and I am inclined to the opinion that this cause may account for all the cases. If the appendix be examined *in situ* before its relations have been disturbed, in quite a number of cases the obstruction will be discovered; in others, on the other hand, the cause of the interference with the drainage may have been swept away. If we have a patient not susceptible to sepsis, there may have been only temporary obstruction of the appendix, and only slight constitutional symptoms; while in another, with a similar obstruction, there may have been marked sepsis and great depression from the first, and the case may become rapidly fatal unless relieved by operation. In most cases of stricture no benefit can be expected from preliminary measures of treatment.

I think that the preliminary treatment is often worse than useless, and that the measures adopted in the very beginning are of extreme importance as regards the prognosis. A common procedure is the administration of a large dose of morphine, which quiets the pain and relieves the distress of the patient, but masks all symptoms. This prevents the proper study of the early stage of the complaint, the stage which is the most important both as regards diagnosis and treatment. The question of the rapid or gradual increase in severity and the advent of sepsis are obscured by the opium-treatment. A patient presenting himself with severe abdominal pain, whether he has fever, nausea, vomiting, prostration, and constipation, or not, should be suspected of having appendicitis. The most probable cause of sudden, severe, and not otherwise easily explained intra-abdominal pain, is appendicitis. Such a patient should be put to bed and thoroughly examined; very little opium or other anodyne should be given, but the patient must be kept at rest and most carefully watched. If the diagnosis can be made during the first six hours, and the patient can be kept under observation for six hours longer, the progress of the disease may tell the whole story. Certainly, in twenty-four hours from the beginning of the attack, we may be able to decide not only as to the diagnosis, but as to the probable course and result of the case. In my opinion, many of the casualties in the disease are due to neglect of the precautions which I have just recommended. The earlier the choice can be made between operative and palliative procedure the better for the patient.

The question of the proper time to operate in an acute attack of appendicitis would call for a week's discussion, and cannot be answered dogmatically for every case. Indeed, no two cases are alike, and each must be treated according to the indications. Certain well-marked symptoms must be watched for and their value carefully determined in each instance. Let us suppose an average case. The patient has had one or two attacks of vomiting; the pulse is only moderately accelerated, and is of good volume and strength; the temperature is about 100° F.; there is abdominal pain, perhaps quite severe, at first diffused, then settling into one point about half-way between the anterior-superior spine of the ilium on the right side and the umbilicus. If in five or six hours there is no increase in urgency, the patient is not in immediate danger; if in twelve hours there is still no increase in the severity of the symptoms, the patient should soon begin to improve. On the other hand if the urgency of the case has steadily increased in twelve hours from the time when we were able to make the diagnosis, an operation will probably be called for. If I can safely allow a patient to get over an acute attack before operating, I always prefer to do so. If in twenty-four hours from the beginning of the attack the symptoms lessen in severity, I usually feel sure that I can leave the case to nature for the time being, and defer my operation to a more favorable period. Such an attack ought to subside in from forty-eight to seventy-two hours. If, however, during the second twenty-four hours of the attack I am doubtful of the outcome, I always advise an operation. This is of course only an imperfect answer to the question, when to operate.

In a given case, the practitioner gets his idea of the course of the attack in one way, and another in another; one relies more upon the facies, another upon the pulse, and a third upon the patient's strength. Here of course experience is of the greatest value. Just as soon, however, as the conclusion is reached that the disease is progressing, the time has come to operate.

I have been asked if I operate on all cases of appendicitis in which the symptoms have practically disappeared, and I have replied that my feelings have changed so much of late that I feel quite willing to do so. Treves lays down the rule that when attacks have been very numerous, and when they have been increasingly severe and threaten life, operation should be done. My own view is that after two attacks a patient will as surely have the third as that after ten attacks he will have the eleventh. There is no rule which applies to the severity of the succeeding attacks. We know, however, that each attack renders an operation more and more difficult, and that it requires a larger incision and involves greater injury to the abdominal wall.

As to the success of the operation itself, all the advantages lie with the operation that is done between the acute exacerbations of the disease. In such an operation we may look for a convalescence of from two or three weeks, unaccompanied by danger from sepsis, or the occurrence of a ventral hernia, and the mortality is almost nothing. In an operation during an acute attack, on the other hand, the prognosis is more grave. The period of convalescence will probably last for about six weeks, and there is decided danger of sepsis and of a subsequent hernia.

The decision of this question, namely, whether or not we can safely allow an attack to go on, and defer the operation, calls for the best possible judgment and experience. In operating between the attacks there is, moreover, a safe time and a bad time. No matter how favorable the patient's condition may seem to be, it is rarely safe to undertake an operation in less than two weeks after an acute attack, for the reason that although the subjective symptoms may be favorable, the local septic condition may not have been entirely removed. After the lapse of two weeks or more all congestion, inflammatory thickening, and danger of sepsis will have disappeared.

I always use a gauze-drain in cases of general septic peritonitis; then, in addition to abundant gauze-drainage, I use a long glass tube, which extends down to the floor of the pelvis. I use a ten per cent. iodoform-gauze. I have never seen iodoform-poisoning after packing the abdominal cavity with iodoform-gauze.

Formerly, I was more willing to operate during the attack than I am now. My feelings have changed, because now we can offer the patient an operation without danger of hernia, if done between the attacks, and this, in addition to the other advantages of the intermediate operation, has led me to prefer it, and to always endeavor to obtain it.

What is the cause of death in appendicitis? In the acute cases, in which operation is done as soon as the diagnosis is made, it is very rare that the result is fatal. This is true of operations in the hands of all operators who have reported their cases. Also when done in the intervals of attacks, operations are enormously successful. Judging from the results, this operation seems like a light and easy thing. Operations done on patients after two or three, or even four weeks, are very successful. When an operation is undertaken on a patient who has suffered from very numerous attacks of inflammation, when there are adhesions and broken-down tissue to embarrass the surgeon, and to increase the risk of sepsis, we get an occasional bad result.

The chief cause of death is, therefore, delay of one sort or another. If the cause of death be general septic peritonitis, this comes late in the attack, and after the time when a good diagnostician should have recognized the disease; therefore, had there been no delay, there would have been no septic peritonitis, which is, as you know, one of the principal causes of death in appendicitis. In the cases which have gone on to the formation of abscess, each day's delay increases the difficulty and the danger of an operation, which at first might have been easy and safe, so that finally it may become an impossibility. Again, then, delay is the principal cause of death.

After an abscess has formed, the proper time to operate has given rise to considerable discussion. Treves, for instance, maintains the view that when suppuration has been walled in, the longer one waits before operating the nearer the pus will come to the surface. This is, in my opinion, an unscientific and unwarranted statement, because instead of approaching the surface the pus may be traveling upward toward the liver or downward into the pelvis. My own opinion is, as I said before, that in abscess-cases the sooner the operation is done the better. In such cases, if the appendix has not yet softened and broken down, and where only a small area of the peritoneum has become involved, the organ can be easily removed in most instances; but in older cases, where we meet with more advanced peritonitis, with adhesions and formation of much pus, the softened and broken-down appendix may be unrecognizable, or if recognized it cannot be safely handled or removed. An abscess opening into the gut results in one of two ways: either the disease is cured, or the appendix remains and another abscess is set up, which may discharge in the same way. I would not operate in such cases unless the abscess recurred.

This brings us to the question of the removal of the appendix in every case. A discussion upon this point has been carried on in Philadelphia, and many operators hold that the organ should always be searched for until found and then removed. In point of fact, in some cases the appendix cannot be found; it may be hidden by a thick and more or less impenetrable wall of inflammatory tissue, and when this happens, it is frequently better to give up the search for fear of making an opening into the intestines. In many cases, too, the search will consume too much valuable time, and may lead to needless and dangerous enlargement of the wound. If the wound is thoroughly cleansed and free drainage established, little is to be feared from remnants of the appendix left in the body. If such a piece of tissue gives rise to trouble, and sets up an abscess it can be operated upon secondarily. I remember a case in which death followed an ill-advised search for the appendix. An abscess-cavity about three inches in diameter had been opened, but the general peritoneal cavity was intact. As the operator did not discover the appendix, he took a sponge and so vigorously wiped the abscess-wall that he broke it, and through this rent a loop of intestine protruded, and immediately drew back into the abdomen, but in that instant it had become infected, and it gave rise to a fatal septic peritonitis.

As to the sequelæ of the operation. I have not found fecal fistula so serious a complication as I feared it would be. Nearly all of these fistulæ close spontaneously. The escape of a little feces is not usually dangerous. Where these fistulæ will not close, if anything is to be done it should be done very deliberately and carefully. The only safe procedure is to enter the peritoneal cavity at some other point than through the fistula. If the fistulous opening is enlarged the intestine is pretty sure to be wounded, but an opening, say a little above the fistula, is safe and easy.—The Medical News.

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PROCEEDINGS OF SOCIETIES.

RICHMOND ACADEMY OF MEDICINE AND SURGERY.

Regular meeting, November 24, 1896.

Dr. Landon B. Edwards, president, in the chair; Dr. Mark W. Peyser, secretary and reporter.

Dr. J. N. Upshur read a paper on "Puerperal Eclampsia." [See page 70.]

DISCUSSION.

Dr. Jacob Michaux said he was sorry he was not present to hear the whole of the interesting and instructive paper, but he wished to speak of several things of which no mention had been made:

First, as to the causation of puerperal convulsions. Conclusions as to the retention of urea in the blood were drawn from the experiment in which one hundred grains of that substance were injected into an animal without causing convulsions. He would say that, considering the amount of urea voided, the quantity was by far not large enough to produce the symptoms. A second theory of causation is the retention of other excreta, which is doubtless true, but he believed urea played an important part.

Regarding the morphine treatment, he was in thorough accord with Dr. Upshur. At one time it was thought proper, but considering its effect on the excretory organs, it could be immediately seen that the indications were thwarted.

A subject in which there was a great deal of misconception was when convulsions were threatening, the custom was to use diuretics. The practice is wrong for this reason: The uterus presses not necessarily on the renal, but on other abdominal vessels, the pressure being projected through intervening ones to those of the kidneys which become congested, and albuminuria results. Using diuretics, the kidneys are further congested. Therefore it is highly important to look to the bowels as the chief means for getting rid of the poisonous matter. He was confident that he had seen impending convulsions thwarted by the use of salines. He coincided, also, with Dr. Upshur in the use of the lancet and chloroform; but employing the former, care should be had in anæmic cases. He had had good results, too, from veratrum viride in conjunction with chloroform, as likewise from chloral and the bromides.

Dr. H. Stuart MacLean stated that Flint records an experiment in which a pound of urea was injected without production of convulsions; but the injection of urine produced them.

Concerning treatment, he said that during his service at the Brooklyn Hospital, of seven cases of puerperal eclampsia, three in which veratrum viride had been administered died, while four not treated thus recovered. In the latter hot packs and bleeding were the measures employed. Where bleeding was done antepartum high injections of saline solution equal to the amount of blood drawn and lavage of the stomach were employed.

Dr. C. R. Robbins described a case in which there were all the premonitory symptoms of eclampsia. Primipara, eight and a quarter months pregnant, urine albuminous, frontal headache, epigastric pain, œdema, spots before the eyes, etc. The treatment consisted of an absolute milk diet, lithia water and nitroglycerine. The patient had no convulsions, and delivery was successful.

Dr. W. W. Parker concurred almost in toto with Dr. Upshur, but had more faith in veratrum viride. He described the following cases:

Case I was in the second month of pregnancy, and when seen before the attack, there was no sign of trouble. Suddenly, there was violent headache, and in half hour convulsions. She died in twenty-four hours without recovering consciousness. He thought her death was due to apoplexy.

Case II.—Eclampsia occurred in the third month of pregnancy. The urine was loaded with albumin. The convulsions were treated, and while they never returned, two weeks after the attack, there were headaches, blindness, etc., and the albumin had increased until the urine was almost solid. Immediate production of labor was insisted upon. He thought the patient could not possibly have survived had it not been done.

Dr. John F. Winn asked if it was Dr. Upshur's custom to give pilocarpine, and if he had ever used the hot pack.

Dr. Upshur replied that the administration of pilocarpine depended on circumstances. As to the hot pack, he never used it, as it was almost impossible in private practice. It was not because of lack of faith that he did not use it.

Dr. Winn continuing said there was one phase of treatment that he had not heard touched upon, and that was early emptying of the uterus. When convulsions were present there was no difference of opinion. When imminent and not yielding to ordinary treatment they would end in premature labor. Therefore, following nature's plan, would it not be well to induce it, thus putting a stop to the convulsions and relieving the woman of a liability to further attacks?

He described a fatal case in which convulsions did not occur until an hour after labor. The hot pack, venesection, chloral and bromide injections were all employed. He thought the induction of premature labor was always called for both in the interest of mother and child in every case whose symptoms were not relieved by dietetic and medicinal measures.

Dr. W. T. Oppenhimer thought the causation had a great deal of bearing on the case. Thus short women are more susceptible than others. Always there is some dyspepsia of the small intestine giving rise to fermentation, which, with the gravid uterus, presses on the blood vessels of the kidneys, producing atrophy. These cases he treated by anti-ferments, digestives, etc. Salines were employed also. Women not disposed to alimentary troubles are seldom affected. He advocated bleeding and the use of alkalies.

Dr. Arthur Jordan, apropos of the injection of urea, remarked that the experiments had been made with rabbits, and that it was supposed, too, that the urea passed off too rapidly. The venal arteries were ligatured but no satisfactory results were obtained.

He thought the trouble was due to hypersensitiveness of the nervous system, no matter how it may be brought about. It behooves us, therefore, to watch anything that has a deleterious effect. The treatment went to favor his statement—viz.: chloroform, chloral, bromides which kept in subjection the nervous system. A remedy he saw used with happy results was morphine in one-half grain doses until a grain and a half to two grains were given.

Dr. Edward McGuire reported the following case: The patient was somewhat anæmic and in the eighth month of pregnancy. She progressed well until July, when, without known cause, there was paresis of the corner of the mouth, uvula and tongue. The diagnosis was embolism of the vertebral artery. Since that time there had been excessive nervousness, the least movement of the child bringing on a nervous attack. In September, while out of town, she had a convulsion, and the physician called in gave chloral. He saw her three hours later. There was no albumin in the urine. A short time ago, two months after the first, she had a second attack in the night. She had complained to her husband of pain in the face, and a feeling of impending convulsions. He saw her when the attack had passed.

As she suffered from photophobia the light was low. Entering the room, he turned on the gas, and she immediately went into another convulsion. He doesn't know what form it was. Since this she has had no other attack. The amount of urine was normal, and there was no albumin. He hesitated to induce labor. She never had a convulsion before pregnancy.

Dr. Hugh M. Taylor said that venesection and transfusion appealed more to him than any other treatment. We want no better evidence of the uncertainty of treatment than its wide range. He reported the following case in advocacy of Dr. Winn's early induction of labor. The patient was short and fat. In the morning there was one pain, and the bag of waters broke. In the early night she was very nervous, but recovered, and labor went on slowly, there being progression and recession. Suddenly there was a terrific convulsion. Chloroform was administered, and she laid in stupor so long that it was hard to tell when the stertor of the convulsions stopped and that of chloroform began. The uterus was emptied, and the patient recovered consciousness; and is now doing well. He did not mean to say that this sustained in toto the treatment. Albumin was never found.

Dr. W. S. Beazley described the following case, a II para. On reaching it he found labor far advanced, with no untoward symptoms, and everything went on normally. He left the house, but in two hours, having been sent for, he found the patient in a hard convulsion, which passed off in a few minutes. She did not regain consciousness, and, in a half hour, went into a second attack. Enemata of bromide of potassium and chloral were given, and chloroform administered, but to no avail, as she grew worse and died. There were no convulsions with the first labor.

Dr. Upshur, in conclusion, remarked that he wished to say that Dr. Oppenhimer had gotten the cart before the horse. The digestive symptoms of which he spoke, and the consequent accumulation of gas, were due to an attempt on the part of the stomach and bowels to relieve the system by vicariously eliminating effete matters which should go off by the kidneys; the irritation thus set up caused the indigestion and consequent flatus. The frequent vomiting, the renal asthma, etc., which we see in several forms of Bright's disease is illustrative, the mucous lining of the stomach and bowels, and bronchioles, seeking to help the kidney. The pregnant woman's blood is in a condition of increased plasticity; there is over-nutrition and over-stimulation of the kidney; it thus fails in its eliminative function, and the presence of albumin in the urine is the external manifestation of this condition; toxic matters remain in the blood, poisoning the nervous system, and find expression in a convulsive explosion when the proper reflex comes, from an overloaded stomach, or the pains of labor.

ABSTRACTS.

WIDAL'S SERUM TEST IN THE DIAGNOSIS OF TYPHOID FEVER.

We have already published an article referring to this, but the great importance of all to do with typhoid in India, leads us to notice now a full review of the subject published in the Journal de Médecine de Paris.

Widal's test is founded on the observations of Pfeiffer and Koll and of Max Gruber, who had remarked that the serum of animals immunized against typhoid, and of man convalescent from typhoid, when mixed in vitro with a culture of Eberth's bacillus, has a reaction enabling us to distinguish that germ from coli-bacillus.

Widal describes the reaction as follows: "If tubes of bouillon be "sown" with Eberth's and with the coli-bacillus separately, and if a few drops of the serum from animals strongly immunized against typhoid infection, be then added to each tube and the tubes be then placed in a stove at 37° C., the following is seen to occur: During the four or five first hours the coli-bacillus bouillon becomes "troubled," while that containing Eberth's bacillus remains almost clear. At the end of 24 hours the contents of the tube "a coli" is seen to be strongly troubled throughout, while that containing Eberth's bacillus is very little troubled, if troubled at all, and the microbes in it are seen to be precipitated in the form of flatus or white pellicules at the bottom of the tube. A drop of the coli-bouillon examined then under the microscope, will show the bacteria quite separate from each other, and for the most part very mobile, while in a drop of the Eberth's bouillon will only be seen disjoined agglomerations of immobile, deformed microbes, glued to one another. "It is this formation of a precipitate visible to the naked eye, this inoculation, agglomeration and distortion of the contained microbes that is the distinguishing feature in the test."

Widal, then, put this reaction to a clinical use. For this but little in the way of instruments or appliances are required. Dieulafoy has said: "It is only necessary to have a pure culture of Eberth's bacillus in bouillon —and that will keep for weeks—a microscope with immersion lens and a few drops, or even one drop, of serum from the blood of the person tested."

The simplest way of operation is as follows: The palmar surface of the finger end of a patient is aseptically cleaned, and pricked then with some instrument previously sterilized. Any blood that exudes should not be allowed to flow over other parts of the finger, but should be received directly into the "sown" bouillon, in the proportion of one drop of the blood to six drops of the bouillon. This proportion, $\frac{1}{16}$, seems the best, but one of blood or serum in sixty of the bouillon has been found to give excellent results.

That, then, is the method at its simplest; but modifications have been proposed, giving more defined results and yet not losing much of this simplicity.

For instance, it has been found better to test the serum instead of the blood complete, and for that, instead of receiving the blood directly into the germ-bouillon, we collect it in a sterilized test tube. The serum which separates almost at once we draw off then, with a sterilized pipette, and we use one part of that serum to ten of the germ-bouillon as before described.

Again, instead of blood serum, we may use the serous flow from blisters. Widal and Sicard obtained equally good results with that.

As to the period of the malady at which the serum is capable of giving this indication, Widal, though he had seen it on the fifth day, tells us that we must not depend upon it before the seventh or eighth day, and that from then it continues throughout convalescence.

Exceptionally it is seen in individuals who have been for a long time cured of the fever. Widal found the reaction in two persons who had suffered from very severe typhoid, three and seven years before, respectively. It is necessary to remember this, but given the rarity of multiple attacks, such occasional occurrence is really of small importance.

In all cases for this test, two bouillon cultures should be used to only one of which the serum is added, while the other is kept for comparison.

Widal states: "If on microscopical examination at first, I see groups of confluent bacteria distributed throughout the preparation, resembling islets in an archipelago, my diagnosis is made. If, however, the appearance is not at once characteristic, or if I find isolated moving bacteria, I leave the mixture to be examined again after some hours, as by then frequently the phenomenon, though not declared at first, has become most distinct. If results are negative, I repeat the test at intervals of a few days, so long as the symptoms last."

With very old cultures, even a few days old, it is well to renew them by sowing again in a pure bouillon. In less than twenty-four hours then, these new bouillon cultures will have become sufficiently "troubled" to give the reaction plainly and at once, upon addition of a typhoid serum.

The cause of this reaction is still under discussion. As to its clinical value we conclude by quoting Widal again: "A negative result obtained with the serum of a patient gives probability against a diagnosis of typhoid. This is only probability, however, and is least to be depended on when the test is applied during the first few days; and the examination should always be repeated at intervals."

"An" *agglutination* "obtained with the serum of a patient without history of previous affection is a certain sign of typhoid fever."—Indiana Medical Record.

ON THE DISINFECTING ACTION OF SODIUM HYPOCHLORITE.

By E. KLEIN, M.D., F.R.S.,

Lecturer on Physiology at St. Bartholomew's Hospital.

In the Journal of the Society of Chemical Industry, Vol. XV., No. 5, Dr. Clayton draws attention to the disinfecting action of solutions of sodium hypochlorite, and describes experiments conducted by himself and Professor Boyce, of University College, Liverpool, which show that sodium hypochlorite even in weak solutions—e. g., containing only 0.2 or even 0.1 per cent. of available chlorine—has a decided disinfecting action on non-sporing microbes. The sodium hypochlorite which was used in the experiments of Dr. Clayton and Professor Boyce, and with which I have also made a series of experiments confirming their results, is a fluid of strong bleaching power, and is described as containing 10 per cent. available chlorine. It was used in my experiments as solutions in distilled water—1 in 10, 1 in 100, and 1 in 200—and was tested on the following microbes: (1) proteus vulgaris; (2) bacillus coli communis (these two representing typical putrefactive microbes); (3) bacillus typhosus; (4) bacillus diphtheriæ; (5) bacillus of swine fever; (6) vibrio of cholera Asiatica; (7) staphylococcus aureus or ordinary pus; (8) spores of bacillus anthracis; and (9) spores of the anærobic bacillus enteritidis.

In the case of the first seven microbes cultures on the surface of nutrient agar were made and kept at 37° C. for forty-eight hours; there was by this time copious typical growth in a very active state. The spores of anthrax were derived from an agar surface culture; when transplanted to gelatine or agar they produced typical growth and when injected into the guinea-pig caused fatal anthrax within thirty-six hours. The spores of bacillus enteritidis were derived from an anærobic culture in grape sugar gelatine that had been established about ten days; the culture was by this time full of spores which, when transplanted into fresh sugar gelatine, yielded normal active growth. The disinfectant fluid—i. e., the hypochlorite of soda—was used in solution; a definite quantity of the fluid is dissolved—i. e., poured into a definite quantity of sterile distilled water kept in a sterile glass-stoppered bottle. Immediately after a small quantity, generally 5 c.c., of this solution is poured into a sterile test tube, and then a particle of the microbe culture above mentioned is taken up with a sterile platinum loop, introduced into the 5 c.c. of the disinfectant solution, and well shaken so as to form a uniform distribution or emulsion. In all instances the emulsion contained the microbes in very great numbers, being distinctly turbid. By making a similarly turbid emulsion in sterile distilled water, and then making plates to ascertain approximately the number of microbes present in the emulsion, it was found that one platinum loop of the emulsion yielded colonies far too numerous to be counted.

After exposing the microbes in the above emulsion to the disinfectant fluid for a definite time, cultures are made so as to see whether, and to what extent, the microbes have been affected. Of the first eight microbes—viz.: (1) proteus vulgaris, (2) bacillus coli, (3) bacillus typhosus, (4) bacillus diphtheriæ, (5) bacillus of swine fever, (6) vibrio of cholera, (7) staphylococcus aureus, and (8) spores of bacillus anthracis—the inoculation of the sub-culture was made from emulsion into nutrient broth and on to nutrient agar and then incubated at 37°C. for four days. By this time any living microbes introduced into the sub-culture would have had ample time and opportunity of producing easily visible growth. In the case of the anærobic spores of bacillus enteritidis the sub-culture was made anærobically in deep sugar gelatine and incubated at 20.5° C. for six days. In every instance the sub-culture was made by introducing into the culture medium three platinum loops of the emulsion so as to transfer as many microbes as possible from the disinfectant emulsion. As stated above, each loop would hold an uncountable number of microbes.

An important comparative experiment was made at the outset. It was this. By introducing from the disinfectant emulsion three loops of the fluid into the culture medium it might be said that this culture medium received a comparatively large addition of disinfectant which might interfere with—i. e., inhibit—the subsequent growth of the microbe transplanted into the new culture from the disinfectant emulsion, and therefore if no growth occurred in the sub-culture this absence of growth might not be due to the fact that the microbes had been previously killed, but rather that although still living, they were incapable of growing owing to the addition to the culture medium of three loops of disinfectant fluid. In order to ascertain whether the addition of three loops of disinfectant emulsion to the new culture medium had any such inhibitory action on the subsequent growth of microbes a series of cultures were made in the following manner: Of disinfecting mixtures (1 in 10, 1 in 100, and 1 in 200—that is, in the strength in which they were used for disinfecting purposes, as will presently be shown), five loops were transferred to each broth and agar in test tubes, then these test tubes were inoculated separately with the above microbes 1 to 8, and incubated. The result was that in all tubes normal and active growth took place, thus showing that the addition of five loops of the disinfectant mixture per culture tube, even of the strength of 1 in 10, had no inhibitory effect on the subsequent growth of the microbes. The experiments on disinfection made with the above nine species of microbes were as follows:

Series 1.—The hypochlorite of soda was used as mixture of 1 in 10—i. e., containing 1 per cent. of available chlorine. The following microbes were used: proteus vulgaris, bacillus coli, bacillus typhosus, bacillus diphtheriæ, bacillus of swine fever, vibrio of cholera, staphylococcus aureus, spores of bacillus anthracis, and spores of anærobic bacillus enteritidis. 1. After exposure for twenty minutes the result was that in no sub-culture was there a growth; the microbes had been devitalized by the mixture 1 in 10 in twenty minutes. 2. After an exposure for ten minutes the result with the same pipe species of microbes was that there was no growth in the case of the first seven species; also in the case of the spores of the anærobic bacillus enteritidis there was no growth; but in the case of the spores of the bacillus anthracis there was a limited growth in the broth culture, and in the agar culture there was one single colony. On sub-culture this yielded normal growth, which produced typical anthrax in a guinea-pig. While, then, 1 in 10 completely devitalized the spores of anthrax in twenty minutes it did not quite succeed in doing this with all the spores, although it did so with the great bulk of them. 3. The above seven microbes were also killed by an exposure for five minutes, no sub-culture yielding growth. The spores of bacillus anthracis and of the anærobic bacillus enteritidis were seemingly unaffected, since copious normal growth was obtained in sub-culture

Series 2.—Hypochlorite of soda used as a watery solution, 1 in 100—i. e., 0.1 per cent. of available chlorine. 1. The following bacilli—the proteus, coli, typhosus, diphtheriæ, swine fever, cholera, and staphylococcus—were devitalized after an exposure for twenty minutes, there being no growth in the sub-culture. The spores of anthrax and of enteritidis yielded normal and abundant growth. 2. After an exposure for ten minutes the same result was obtained as above. 3. Also after an exposure for five minutes the same result was obtained. It follows, therefore, from this series that hypochlorite of soda in solution of 1 in 100 disinfects the non-sporing microbes above mentioned in five minutes, but does not do this with the spores, anthrax and enteriditis in twenty minutes. 4. For the spores of these two microbes the time of exposure was then prolonged to (a) thirty minutes, (b) one hour, (c) one and a half hours, and (d) two hours. The result was that the anthrax spores exposed for thirty minutes to the disinfectant (1 in 100) yielded in sub-culture normal and copious growth both on agar and in broth. The same spores for one hour yielded no growth in broth and a limited number of colonies on agar. After exposure for one and a half and two hours no growth took place. Spores of bacillus enteritidis exposed for thirty minutes yielded good growth. The same spores exposed for one hour yielded a very limited number of colonies. When exposed for one and a half to two hours no growth was yielded. From these experiments it follows that the hypochlorite of soda used as solution of 1 in 100 had a complete disinfecting power on both anthrax spores and enteridis spores when acting for one and a half hours; its disinfecting power was limited when acting for one hour, but it had no effect in thirty minutes. This result of the solution of 1 in 100 on spores of anthrax (complete disinfection in one and a half hours) compares most favorably with all disinfectants that I am acquainted with, exclusive, of course, of mercuric bichloride.

Series 3.—Hypochlorite of soda used as a watery solution, 1 in 200—i. e., 0.05 per cent. of available chlorine. In this series, also, the time of exposure was (1) for twenty minutes, (2) for ten minutes, and (3) for five minutes. The microbes used were: proteus vulgaris, bacillus coli, bacillus typhosus, bacillus diphtheriæ, bacillus of swine fever, vibrio of Asiatic cholera, and staphylococcus aureus. All these succumbed to the action of the solution after a five minutes' exposure, so that no sub-cultures were obtained in any of the culture tubes. Of course, exposure for ten and twenty minutes respectively had *a fortiori* the same result. On the spores of bacillus anthracis and those of bacillus enteritidis this solution 1 in 200 had practically no effect, as even after exposure for four hours good and copious sub-cultures were obtained.

It follows, then, that on non-sporing microbes the solution of hypochlorite of soda (1 in 200) acts as disinfectants in five minutes. Weaker solutions that I have tried—e. g., 1 in 300 and 1 in 350—required an exposure so long that for practical purposes it must be considered as not applicable, the bacillus coli, and particularly the staphylococcus aureus, exposed to the solution 1 in 300 for three hours yielding in sub-culture good and normal growth. An indication of the positive disinfecting action of the solution is hypochlorite of soda on the microbes in the above experiments is noticeable already by simple inspection of the microbic emulsion. It is this: the emulsion becomes changed from the original turbid into a clear solution; this clarifying action is in proportion, in point of time, to the strength of the hypochlorite of soda solution. Using it in the strength of 1 in 10 its clarifying action is already noticed in from two to three minutes on the emulsion of non-sporing microbes; an emulsion of spores of anthrax is transparent already in ten minutes.

Series 4.—In this series sewage as passed out of St. Bartholomew's Hospital was mixed with hypochlorite of soda. A measured quantity of the sewage (99 c.c.) was put in a glass-stoppered sterile bottle, and to it was added 1 c.c. of the disinfectant. Consequently we had there a solution of 1 hypochlorite of soda in 100. The untreated sewage was tested for the number of living microbes contained in it, which was found to be a little under three millions per 1 c.c. After shaking up the 99 c.c. of sewage with 1 c.c. of hypochlorite of soda sub-cultures with three platinum loops per tube were made in broth, on agar, and for anærobes in deep grape sugar gelatine. The first two were kept at 37° C. for four days and the sugar gelatine culture at 20.5° C. for six days. 1. After thirty minutes all the culture tubes remained free of growth. 2. After twenty minutes there was no growth in the ærobic cultures, but there was one colony in the anærobic culture; this proved to be a colony of bacillus butyricus. 3. After ten minutes there was copious growth on the agar surface, which proved to be a pure culture of the sporing bacillus mesentericus, but no growth in the broth culture. It follows from this that all non-sporing microbes of the sewage were killed in ten minutes by the addition of 1 c.c. of hypochlorite of soda to 99 c.c of sewage. Spores, however, both of the ærobic bacteria (bacillus mesentericus) as well as the anærobic (bacillus butyricus) were not disinfected in ten minutes, but appeared to have been destroyed in thirty minutes. This last result does not quite coincide with the results obtained with the spores directly experimented with in the former series (Series 2), in which 1 in 100 of disinfectant did not harm the spores of anthrax of enteritidis. The spores of bacillus mesentericus having a greater resisting power to inimical influences (heat and chemical disinfectants) than those of bacillus anthracis, and the latter withstanding the action of hypochlorite of soda, 1 in 100, for thirty minutes, the above result of the apparently successful disinfection of the spores of the bacillus mesentericus in twenty minutes seems at first sight unintelligible. It is, however, explained if we remember that in the experiments with the spores of anthrax (Series 2) three loops of the emulsion would contain an enormous number of spores, while three loops of the sewage may fail to contain a spore of the bacillus mesentericus, this microbe, or its spores, being present far less numerously in the sewage than in our emulsion crowded with the spores of bacillus anthracis.

The addition of 1 hypochlorite of soda to 100 of the fluid is sufficient to devitalize non-sporing microbes in sewage or any similar fluid in ten minutes.—The Lancet.

A CASE OF ULCERATIVE ENDOCARDITIS WITH ANTISTREPTOCOCCI-SERUM; RECOVERY.

By DR. SAINSBURY.

The patient was a lad of thirteen years, who had been sick for more than two weeks, complaining of pain in the chest, and suffering from delirium and some fever. Upon admission the area of cardiac dullness was found to be enlarged, and there were a thrill and double murmur at the apex. The breath-sounds were weak and there was crepitation in both lungs. Four days after admission a symmetrical erythematous rash appeared on the buttock and spread to the rest of the body; and five days later streptococci were found in the blood. The symptoms now grew irregularly worse, until the patient had relapsed into a state of extreme cachexia. In this condition, more than two months after the onset, treatment with antistreptococci-serum was commenced. The first dose was of twenty cubic centimeters, and within a week forty cubic centimeters more had been given. There was no reaction, but the condition of the patient improved very rapidly until he was able to sit up, although the temperature curve was not absolutely normal. Six days after the previous series of injections, therefore, a final injection of ten cubic centimeters was made This was followed by a rise of temperature to 102° F., malaise, and considerable pain at the site of the injection. After this the temperature remained normal, all the physical signs disappeared, and the patient was discharged cured.—Lancet, October 17, 1897.

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THERAPEUTICS.

GRANULAR CONJUNCTIVITIS.

Mercuric oxide	gr. iij
Zinci,	
Thymol,	
Muriate of cocaine	aa. gr. ss
Camphor	gr. ss
Vaseline......................	oz. j

M. ft. ung.—North American Practitioner.

TEN HYGIENIC APHORISMS.

The late Dr. Frank H. Hamilton, of Bellevue Hospital, is said to have framed the following decalogue of health precepts: (1) "The best thing for the insides of a man is the outside of a horse. (2) Blessed is he who invented sleep—but thrice blessed the man who will invent a cure for thinking. (3) Light gives a bronzed or tan color to the skin; but where it uproots the lily it plants the rose. (4) The lives of most men are in their own hands, and as a rule, the just verdict after death would be—felo de se. (5) Health must be earned—it can seldom be bought. (6) A change of air is less valuable than a change of scene. The air is changed every time the wind is changed. (7) Mold and decaying vegetables in a cellar weave shrouds for the upper chambers. (8) Dirt, debauchery, disease, and death are successive links in the same chain. (9) Calisthenics may be very genteel, and romping very ungenteel, but one is the shadow, the other the substance, of healthful exercise. (10) Girls need health as much—nay, more than boys. They can only obtain it as boys do, by running, tumbling—by all sorts of innocent vagrancy. At least once a day girls should have their halters taken off, the bars let down, and be turned loose like young colts."—Medical News.

RESOURCES OF CLIMATE IN HEALTH AND DISEASE.

By S. S. WALLIAN, M.D.

In studying the physiological and pathological characteristics and influences of the different climates on the human economy, the first observed results on experiencing a hot climate may be thus approximately outlined:

1. The circulatory and respiratory systems are immediately but not permanently stimulated, the breathing and cardiac movements being accelerated.

2. The body temperature is definitely increased, the elevation amounting to as much as 18° Fahrenheit, and persisting for years, if not permanently.

3. The appetite is correspondingly increased.

4. The kidneys and sexual system are involved in the prevailing condition of excitation. Puberty in both sexes is inclined to precocity, the animal nature maturing at an early age in comparison with its development in colder climates.

5. There is a marked tendency to enlargement of the liver.

6. The individual loses weight and tends to become spare notwithstanding the increase of appetite.

Nature's two prime tonics, antiseptics, germicides and restoratives are pure air and sunshine. In comparison, all the products of the laboratory and the lists of the pharmacopæia are rubbish and dross. It follows that the value of any climate to the hale man, or to the invalid, is in exact proportion to the opportunity and inducement it extends to him to avail himself of these potent agencies. Besides this crucial test the readings and indications of all the complicated meteorological instruments are scientific shoddy. Climatologists need a comfortometer.

Climate is unquestionably a controlling factor in establishing the death-rate of any community; but the mortuary statistics are proverbially unreliable, since the records are so carelessly kept and so indiscriminately compiled. Official and compulsory records, extending over long periods, will be required to make these tables accurate, or even passably reliable guides. From those at hand, some approximate inferences may, however, be deduced.

The average annual death-rate of the several European countries is thus stated:

France, one in thirty-two; Prussia, one in thirty-nine; Austria, one in forty; Norway and Sweden, one in forty-three; Denmark, one in forty-five; England one in forty-six.

The average annual death-rate for the United States, based on less reliable data than the foregoing, however, has been stated as follows:

Gulf States, one in sixty-three; other Southern States, one in seventy;

Mississippi Valley and Atlantic States; one in eighty; Western States, one in one hundred and fifteen, and Northwestern States, one in one hundred and twenty.

Setting aside a wide margin on the score of incomplete and inaccurate data, and in the face of the fact that various European localities have acquired a world wide reputation for healthfulness, it appears evident that the climatic conditions found in favored portions of our own country, as well as throughout the entire Union are more conducive to health, enjoyment and longevity than any found in Europe.—The American Therapist.

TREATMENT OF LIGHTNING STROKE.

Professor Oliver Lodge writes to the Liverpool Post to warn the public against the notion that a lightning stroke is necessarily fatal. It stops the vital organs, he says, but it rarely destroys them. If respiration can be maintained artificially for a sufficient time, there is a fair chance that the heart will resume its suspended action, and that the stricken man will recover. The practical outcome of this is never to pronounce a lightning-struck person dead until the well-known method of resuscitation from drowning has been practised upon the apparent corpse for two or three hours. Experience has justified this teaching both in America and in France, where it has been strenuously urged and practised by Dr. d'Arsonval. This is a matter of great importance. Comparatively few people are killed by lightning in this country, but it seems probable enough, if we are to believe Professor Lodge, that the number could be still further reduced if artificial respiration were practised.—Br. Medical Journal.

TRAUMATOL IN SURGERY.

In the Journal des praticiens for September 26, M. L. Floersheim calls attention to a new substance which is likely to enter into ordinary surgical practice. It is a substitute for iodoform, the antiseptic qualities of which it possesses even to a greater degree, and has the advantage of being less toxic and exempt from the disagreeable odor which is so unpleasant to patients. The conditions in which it may be employed are the general indications for all antiseptics. Its preparations are as numerous as those of iodoform and its employment is as simple. It may easily be incorporated with vaseline in the following manner:

Vaseline	75 grains,
Traumatol	15 "

This ointment may be employed in cutaneous affections or in the surgery of the eye. Gauze may be impregnated with traumatol as easily as with salol, iodoform and boric acid. Traumatol pencils have been successfully used by certain surgeons in fistulous tracts and in endometritis; their preparation is as follows:

Traumatol	150 grains,
Pulverized gum	8 "

This is to be mixed with a sufficient quantity of water and glycerine. Traumatol may be also mixed with oil and collodion.

This new antiseptic, says M. Floersheim, is endowed with an active microbicidal power; it is inodorous; it does not give rise to erythema or to pruritus; it produces perfect occlusion and hastens cicatrization; and its employment so far has given very encouraging results.—N. Y. Med. Journal.

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MISCELLANEOUS.

THE HEALTH DEPARTMENT AND THE SERUM DIAGNOSIS OF TYPHOID FEVER.—Dr. Hermann M. Biggs, the pathologist and director of the bacteriological laboratory, in a communication, dated November 7th, to the Hon. Charles G. Wilson, president of the city board of health, says:

"I desire to direct the attention of the board to a new laboratory method for the diagnosis of typhoid fever, which, judging from the data available at the present time, promises to be of very considerable practical value in the diagnosis of early or ill-defined cases of this disease.

"The investigations of Pfeiffer and Widal have shown that the blood of persons suffering from typhoid fever, when mixed with active cultures of the typhoid bacillus, has the power of arresting the active movement of these organisms and of producing peculiar and characteristic clumping of the bacilli. It has been shown that this reaction occurs frequently very early in the course of the disease, that it is found throughout its course, during convalescence, and often for a considerable period after complete recovery. It does not occur with other organisms than the typhoid bacillus, and it does not occur with cultures of the typhoid bacillus when the blood of persons suffering with other diseases is employed. As has been shown by Widal and Johnson, this reaction occurs as well with specimens of dried blood as with fresh blood, and thus can be employed practically for the diagnosis of this disease in municipal laboratories. Observations on this matter which have been in progress in the laboratories of this department for some time past have thus far confirmed the conclusions of previous investigators.

In order that more numerous data shall be at the command of this department and that physicians of New York may at the earliest moment have facilities for testing the reliability of the observations thus made, I would respectfully recommend that arrangements be made to place facilities for such examination at the command of all physicians in this city, it being distinctly understood that this action of the department is for the purpose of gaining information on this important subject, at the same time of placing at the command of physicians opportunities for observing the results. If the data already obtained are entirely confirmed by subsequent observations, this method will undoubtedly prove of great service in the diagnosis of early and obscure cases of typhoid fever.

"Should this action be determined upon, circulars of information as to the method of collecting blood and slides for this purpose may be left at the depots already established for the collection of diphtheria culture tubes and the distribution of diphtheria antitoxine."

Accordingly, the following resolution was adopted by the board at a meeting held on November 6:

Resolved, That the recommendations of the director of the bacteriological laboratories of this department, contained in the communication dated November 6, be and are hereby approved, and that he is hereby authorized to place facilities at the command of physicians of this city for the diagnosis of cases of typhoid fever, in accordance with the method prescribed in said communication.

The following notice to physicans has been issued by Dr. Biggs:

"Circulars of information regarding the method employed and slides for collection of blood may now be obtained at the various druggists where diphtheria culture tubes are kept. Physicians desiring to make use of this method in the diagnosis of early or obscure cases of typhoid fever can procure these, and after collection of blood as described, may leave the slides with the data relating to the case, at the various depots. They will be collected each day, examined the following day, and the report of the result forwarded to the physician."

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REVIEWS.

A System of Gynæcology. By many writers. Edited by Thomas Clifford, Allbutt, M.A., M.D.Camb., LL.D., F.R.C.P.Lond., F.R.S., F.S.A., Regius Professor of Physic in the University of Cambridge, Fellow of Gonville and Caius College, and W. S. Playfair, M.D.Edin., LL.D., F.R.C.P.Lond., Professor of Obstetric Medicine in King's College and Obstetric Physician to King's College Hospital. London: Macmillan & Co., Limited. New York: The Macmillan Company. 1896. Price 25s. net.

We had occasion in considering the first volume of Allbutt's System of Medicine to speak of the curious fact that, as a part of the system, a special volume on gynæcology was going to be issued. This volume has now appeared, and constitutes volume II. of the system referred to. Playfair's system of midwifery is already so well known in this country that nothing else would be necessary to give the stamp of merit and worth to a work on a kindred subject, and yet our publishers in this country have shown unusual zeal and great ability in producing gynæcological works in the last few years, and it is not easy to persuade Americans that the foreign point of view in matters gynæcological is at all superior to their own. The editor has himself written very little for the volume, and we confess that we are considerably surprised to find no American selected as a contributor to its contents. On the other hand, the text does not fail to note a number of Americans to whom special credit is due for work in this line. In order, however, to obviate any question of prejudice, we take pleasure in presenting to our readers the review given in the Lancet for November 28, 1896:

"We think the editors have been well advised to arrange in a separate form, in the present instance in one volume, the information on gynæcology which they intended to include in their 'System of Medicine.' As Dr. Playfair says, this department of medicine has grown largely within the last few years, and we are inclined to doubt whether a work of the scope indicated by the title, 'A System,' should not have required one more volume, or perhaps, indeed several more volumes, in order that the whole subject might have been adequately treated. There are several small books on gynæcology, besides many of a size similar to the one before us, and it seems a matter of regret that the opportunity afforded by sub-dividing the labor among twenty-six contributors should not have resulted in the production of an exhaustive dictionary on gynæcology thoroughly representative of the position of that department of practice in the United Kingdom at the present time.

"Dr. Playfair says in the preface that 'it is obvious that a collection of independent essays written by men on topics which they have specially studied must carry more weight and be more useful than any work compiled by a single writer.' No doubt there is a great deal of truth in this. Many of the articles, however, are necessarily on subjects that must be regarded as equally familiar to all competent gynæcologists; such, for instance, are the articles on 'Diagnosis in Gynæcology, the Disorders of Menstruation, Diseases of the External Genital Organs,' and some others, which, excellent as they are, would probably have been equally good if written by any of the contributors other than the actual author in each case. On the other hand the principle referred to in the above quotation from the preface has force as regards the article on the 'Anatomy of the Female Pelvic Organs' by Dr. Berry Hart, in that on 'Extra-uterine Gestation' by Mr. Bland Sutton, in that on 'Pelvic Inflammation' by Dr. Cullingworth, and in some other cases. The article on the 'Etiology of the Diseases of the Female Genital Organs,' by Dr. Balls-Headley, strikes us as one of the least satisfactory in the book; much of it is so much a matter of assumption that, however creditable to the writer's imagination, the result cannot be otherwise than unsatisfactory in a scientific treatise, where conclusions should be based on facts. Take, for instance, the following passage (p. 118), speaking of the 'congenital apparently granular os,' by which the author presumably means a slight erosion. He says:

" 'The effect of this exposure of the glandular structures at the external opening of the cervix to the influences of the acid vaginal secretions, and to friction against the vagina on movement, intensified by fixation due to abnormal abdominal pressure, is the production of an excessive supply of blood, which causes congestion and inflammation of the glands and increased secretion of their strongly cohesive mucus which plugs the canal; the uterine vessels thus becoming enlarged a varicose state may be induced, and the whole uterus become congested, so that general endometritis ensues. Also, the connective tissue at the face of the cervix becomes hyperplastic, the lips are compressed, and thereby the secretions, which are usually plentiful, find difficulty in escape; the uterus becomes irritated by distention so that endometritis is increased and evolutionary disease of the tubes, peritoneum, and ovaries, and (under the concurrent influence of excessive abdominal pressure) anteflexion or retroversion ensue: hence result virginal menorrhagia and dysmenorrhœa and sterility on marriage.'

"Dr. Balls-Headley evidently holds strong views on the subject of stays. Without for a moment denying the danger of tight lacing (it does not necessarily follow that every woman who wears stays laces them too tightly), there can be little doubt that the whole thing becomes a little ridiculous when almost everything, from ulcer of the stomach and jaundice to peritonitis, is gravely ascribed to the habit of wearing stays. The following extract will serve to illustrate the author's method:

" 'The Fallopian tubes are liable to be bent at their junction with the uterus by the misplacement of the uterus in combination with pressure downwards of the intestines by the stays and dress. This in the sexual engorgement in love-making, with or without union, in women of warm appetite, this abnormal relation of the tubes to the uterus may induce effusion of their secretions into the peritoneum, particularly during menstruation, and a local peritonitis; otherwise, they would pass in the normal direction through the genital canal.'

"In the preface Dr. Playfair disclaims all responsibility other than that involved in having selected the contributors; but in doing so we think he has taken an unusual view of the duties of an editor. The editor of a journal who invited contributions, and published everything sent him by his staff, without selection or emendation would probably soon cease to have a journal to edit. In the present instance we think the work would have been improved had the editor taken a somewhat broader view of his functions. The paper by Dr. Sinclair on 'Malignant Diseases of the Uterus,' good as it is in many ways, will no doubt be read with interest; but it appears to us regrettable that in discussing the operative treatment of cancer of the cervix, and the vexed question as to whether cases suitable for operation should be generally treated by total extirpation of the uterus or by the supra-vaginal amputation of the cervix, he has contented himself with quoting Continental statistics and his own statistics only, and has omitted to notice much of the work done by operators in his own country. In an essay such as this on a System of Gynæcology we might have surely expected to find a fair summary of both sides of the case, and if statistics are quoted on one side they should have been quoted on the other. This is the more desirable, inasmuch as series of operations for cancer of the uterus by English operators have been published from time to time—for instance, in our own columns—which, from both the point of view of mortality and non-recurrence, compare very favorably with the series here furnished by Dr. Sinclair from his own practice. In the paper read by Mr. Knowsley Thornton on this subject at the meeting of the British Medical Association in 1895, both sides of the case were fairly stated, and we think in the present instance Dr. Sinclair would have done well to have followed Mr. Knowsley Thornton's good example in this respect.

"In London we have not been hearing much lately of the electrical treatment of the diseases of women, on which we find here a paper by Dr. Milne Murray. As regards the action of electricity on fibroid tumors of the uterus we read that 'Apostoli's method is now generally regarded by those who have given it a fair and intelligent trial as fulfilling a certain well-defined but highly important function in gynæcological therapeutics;' on the other hand, we know at all events that several men who have tried it fairly and patiently have abandoned it altogether, as having little or no practical value, and as not being free from danger. Among the articles not already referred to we observe in that on 'Gynæcological Therapeutics,' by Dr. Amand Routh, a useful chart of baths and health resorts, with an indication of the proper season in each case; while in the article on 'Hysterectomy,' Mr. Knowsley Thornton's avowed preference for the extra peritoneal method has not prevented him from giving a lucid and complete summary of the various alternative procedures.

"The book as a whole, like most specimens of composite work, must be regarded as being a little unequal; while there is little that is new in it, most of the articles are readable (with the distinct exception of that by Dr. Balls-Headley), and the information under each heading appears to be generally sound. In these days when it is becoming customary for expensive books to be beautifully illustrated, we cannot help thinking that the work before us falls a little short of what might have been expected in this respect, if we compare it, for instance, with the somewhat similar American Textbook of Gynæcology edited by Dr. Baldy."

Therapeutics of Infancy and Childhood. By A. Jacobi, M.D., Clinical Professor of the Diseases of Children in the College of Physicians and Surgeons, Columbia University, New York; President of the Association of American Physicians, etc. J. B. Lippincott Company, Philadelphia, pp. 518.

The publication of this work forms an epoch in American pædiatrics. In a compact and extremely readable form, the author has placed before the profession his views on nearly every phase of this subject. While those who have kept abreast of the progress of medicine, and especially pædiatrics, since the author established the first systematic course of clinical instruction in diseases of children, over one-third of a century ago, may find in these pages much that is familiar to them. The more recent graduate will discover in it the cystallization of the bedside experience of the coryphæus of pædiatrics in this country. That the reading of, and constant reference to such a book, will prove of inestimable value to the practitioner is beyond dispute.

In the preface the author expresses, as is always his wont, views quite original and courageously at variance with the ordinary experience of physicians. For instance he says: "I believe in medicines. Advancing years and experience during a period of increasing exactness in medical method have rather strengthened my belief than otherwise." We venture to assert that the majority of physicians discover their belief in drugs to be waning as they advance in years and experience.

Dr. Jacobi has devoted a large part of his book to diet and hygiene, but his strong emphasis of the great value of medicines is sufficiently startling to deserve mention.

The work contains chapters on Feeding of Sick Children; Treatment of the Newly Born; General Therapeutics; Constitutional Disorders; infectious Diseases. Diseases of the various organs are treated in separate chapters.

Throughout the entire book the clear and confident clinician is discernible. The reader cannot fail to be impressed with the faith which the author displays in drug medication. True to his statement in the preface, he is a believer in drugs, for we doubt if another modern treatise or practice can be found in which these play so paramount a role. It is the mission of the doctor to cure; whether he accomplishes his object by drugs or otherwise, matters little; but, cito et jucunde should be the motto of the therapeutist. Contrary to the nihilistic spirit of the day, especially contrary to the German therapeutics, we have in Dr. Jacobi's book a drug therapy which is as interesting and instructive as it is courageous in its avowal. The time may be ripe for a return to active drug medication, from which the leaders of medical thought have gradually been weaned away in the last two decennia. Dr. Jacobi does nothing by halves; his doses of drugs are large, and this may be the reason of his greater confidence in them, the result, doubtless, of large experience. On page 148 he says that "a child of three years may take from ten to twenty-five grains antipyrin daily in from two to four doses, two of which have often to be given in close proximity." The dose of antifebrin is but one-third or one-quarter of that of the former." Quinine, he advises, is for the same age in doses of five to seven grains once a day or every other day, in typhoid fever. Subnitrate of bismuth, from a scruple to a drachm daily, for the diarrhœa of typhoid. Of the infusion of digitalis U.S.P. Dr. Jacobi recommends "for a six-year-old child doses of a teaspoonful two or four or five times a day, the fluid extract (Squibb's), two or three minims daily; digitalis (Mercke), 1-100 gr. three to ten times a day."

On page 335 Dr. Jacobi states that in pneumonia of children he has "not infrequently given the equivalent of from one to four grains of digitalis in a single dose, which was repeated one or more times. To give doses of spartien less than ¼ or ⅛ gr. every hour or two is useless. Such effect as we require we have a right to demand speedily, and the doses must be large enough to enforce it."

These quotations show that the work is unique in many respects, trenchant in statement, bold in the practice inculcated, and far removed from the commonplace text book. As a book of reference, when in need, the physician will surely find it full of resources in the most varied phases. No practitioner may regard himself as well prepared for the diseases of children unless he has carefully read this latest authoritative book.

Pediatrics. Devoted to the diseases of children. Published semi-monthly with illustrations. Van Publishing Co., 1432 Broadway, New York. John Bale & Sons, Great Titchfield street, W. London. Vol. I., No. I. January 1, 1896. $2 a year.

This new aspirant for professional favor appears in bright, readable form, clear type, and altogether as an attractive youngster. So much for its dress. The table of contents gives promise of good, substantial, practical reading.

The leading article being from the pen of America's greatest pædiatrist, on a subject of enormous import, will be scanned with eager interest by the general practitioner. And it will repay perusal and re-perusal because it presents the whole subject of infant feeding in a concise but authoritative form.

A rare case of cretinism, with illustrations, and several other interesting papers by well-known authors, together with items of practical interest, complete this first volume. A large list of American and foreign collaborators have been secured by the editor, one of whom furnishes in the first number a well-written and judicious review of a late work on "Nervous Diseases of Children," gives promise that the high standard evidenced by this first number will be amply maintained. A journal on pædiatrics should contain such matter chiefly as interests the general practitioner. A minor portion of the space only should be devoted to items of special value to the pædiatrist. Upon this line pædiatrics seems to be constructed by Dr. Dillon Brown, who has long been favorably known to the profession as an advanced pædiatrist. We welcome this youthful but sturdy visitor to our exchange list.

ANNOUNCEMENTS.

E. B. Treat, publisher, New York, has in press for issuance early in 1897, the International Medical Annual; being the fifteenth yearly issue of that well-known one-volume reference work. The prospectus shows that the volume will be the result of the labors of upwards of forty physicians and surgeons, of international reputation, and will present the world's progress in medical science.

The publisher states that the kind reception accorded to the Medical Annual has rendered it possible for him to spare no expense in its production; while the editorial staff have devoted a large amount of time and labor in so condensing the literary matter, as to confine the volume within a reasonable size, without omitting facts of practical importance.

The value of the work will be greatly enhanced by the thoroughness of illustration, both colored plates and photographic reproductions in black and white will be used wherever helpful in elucidating the text.

"To those who need the condensed and well-arranged presentation of the medical advances of the past year—and this class must necessarily include all physicians—we heartily recommend the International Medical Annual."

The volume will contain about 700 pages. The price will be the same as heretofore, $2.75. Full descriptive circular will be sent upon application to the publishers.

Victor Kœchl & Co., importers of some of our most valuable modern medicinal preparations are publishing a little monthly entitled "Therapeutic Progress," devoted to brief reports and condensed abstracts pertaining to new remedies. Each issue contains something of practical interest to the physician. We have during the past year numbered this little paper among our exchanges, and can vouch for its practical helpfulness, and also for the ethical and scientific manner in which it is edited.

Any physician who desires to keep informed in regard to the newer remedies, can receive Therapeutic Progress free of charge during 1897 by addressing a request to the publishers, 79 Murray street, New York.

A New Volume. With the number bearing date January 2, The Living Age begins its two hundred and twelfth volume. This sterling magazine loses none of its interest or value, but rather grows in excellence as its years increase—adding the experience of the past with full appreciation of the needs of the present.

This is emphasized by the advanced step it has just taken. If anything more were necessary to make it an ideal magazine, especially adapted to the needs of the American reader, then the additions of a monthly supplement devoted to Readings from American Periodicals, Readings from New Books, and a List of Books of the Month, is that thing.

The general reader is put under further obligations to the publishers of The Living Age for giving them, in addition to what has heretofore been presented in its pages, the above mentioned supplement, translations from the periodical press of Continental Europe. Hitherto this wide field, containing much of interest and profit to the American public, which is so alive to every question of the day, has been unknown ground, to all but the favored few.

The first number of the new year has the following table of contents: "The Olney Doctrine," by Sidney Low; "The Duel of the Period in France," by James Pemberton-Grund; "Bandi Miklos," from the Hungarian, by Selina Gaye; "A Modern Morality," by Jules Lemaitre, from the French; "The Puritan in History," by Principal Fairbairn; "Recollections of Coventry Patmore;" "Catholic Mystics of the Middle Ages," and "A Winter's Walk;" with poetry and fiction.

This, the first weekly issue of the new year, is a good one with which to begin a new subscription. For fifty-two numbers, aggregating about 3,600 pages (300 pages a month), the subscription price ($6.00) is very low. The Living Age, Co., Boston, are the publishers.

Transactions of the Medical Society of the State of North Carolina; forty-third annual meeting, held at Winston-Salem, N. C., May 12, 13 and 14, 1896. Wilmington, N. C. Le Gwin Bros., printers and binders, 1896.

Report of the Surgeon General U. S. Navy, Chief of the Bureau of Medicine and Surgery, to the Secretary of the Navy. Washington, Government Printing Office, 1896.

NEWS AND NOTES.

The thirteenth annual meeting of the Fifth District Branch of the New York State Medical Association will be held in Brooklyn on Tuesday, May 25, 1897. All fellows desiring to read papers will please notify

E. H. Squibb, Secretary,
P. O. Box 760, Brooklyn.

The medical profession of New York city and suburbs and villages have lately been regaled by an invitation from the management of the Roosevelt Hospital to attend the "opening day" of their annex mammoth pavilion for "private pay patients."

This, it seems, has proven the last straw to break the camel's back, for at last our medical contemporaries have shaken off their lethargy and awakened to a realization of the menace to the profession, which these ever-growing, so-called private hospitals are becoming. The Medical News comes out, and in clarion tones, and in most scathing terms condemns this latest enterprise to further plunder practitioners; and in last week's issue of the Medical and Surgical Bulletin, the editor condemns the whole scheme, and warns practitioners to be on their guard against this latest encroachment on their means of existence.

And no wonder, for be it remembered that this concern has an equipment for the "excellent nursing and professional care" of several hundred patients.

All that is needed now to reduce the mass of the profession in New York to a state of pauperism is a million or two from the Vanderbilts to endow it.

Why on earth should any one offer a fee to a surgeon, employ a physician to attend him through an attack of typhoid, pay a nurse or bear the expense of medicines, etc., when the whole thing can be had for the mere trifle of $6 or $7 per week?

The best thing that can be done with this palatial addition to the over-hospital supply of New York is to convert it into a home for the infirm and destitute aged, something badly needed in all our larger cities. As a hospital, or a "hospital boarding house," as one of our exchanges very properly designates it, its doom is sealed, for no such institution can exist when anathematized by a united profession.

Apropos to this it may be said that one of the most potent causes of the late growth of private hospitals and sanitariums for surgical cases has been the humbug theory of antiseptics.

We noticed lately a trenchant essay on this subject from no less an authority than Frederic Treves, who exposes the thin-veiled quackery of certain operators, and shows that the greater part of modern surgical operations in our theaters are performed for dramatic effects; that the antiseptic hobby is being ridden to death, and that any operation may be performed with as good or better chances in one's home as in any hospital.

Let physicians rid themselves of the nightmare of germ infection and utilize the simple principles of common clean linens in surgical operations. Let them insist on these operations being done under the shelter of their own homes, amid the cheer and solace of their kindred or friends.

There is no longer a "corner" in surgical knowledge, as there are few large villages now without one or more capable surgeons.

If the patient be poor let the service on the case be divided, the surgeon operating gratis and the family physician continuing the after-treatment.—Editorial, The Times and Register.

H. Strauss gives the result of an exhaustive bacteriological and partly chemical examination of the gastric contents of a patient suffering from H_2S formation in the stomach.

The presence of the bacterium coli commune was proven bacteriologically, and a series of tests rendered it probable that this bacterium was the cause of the formation of the H_2S in the case reported. Regarding the question under what conditions the bacterium coli causes a production of H_2S, it is interesting to note that in the discharged gastric contents the H_2S development was inhibited by addition of grape sugar, and in place thereof an active CO_2 gas fermentation could be developed. Hence, it appears as if H_2S fermentation will readily occur in the stomach, providing no saccharine carbohydrate is present. This is also the reason of the clinical fact that, particularly in some ectases accompanied by good HCl secretion, H_2S formation is so frequently observed.

Furthermore, the formation of indol was detected in the stomach, and for this also the bacterium coli is responsible.—Berl. Klin. Woch., 1896, No. 18.

At a late meeting of the Medico-Psychological Association of Great Britain, the question of medical secrecy was declared more important to those dealing with insane people than to the general professsion that dealt with the sane. Sometimes the bare fact that a business man had consulted them at all would, if revealed, ruin him for life. They held that "In the opinion of this association, information afforded by a sane patient for a better understanding or treatment of his malady, to a medical man is sacred, and ought not to be revealed without the consent of the patient." This rule, however, they held could not apply in cases of insanity, where the patient was deliberately plotting murder or some other crime. A violation of confidence in such a case would be justifiable. In cases where delicate family affairs are revealed by the insane, strict secrecy should be the rule. Information gained from the insane should be revealed only when it is expedient for the public safety or the patient's own welfare.—Brit. Med. Jour.

Dr. Scheube, of Kioto, Japan, lately suggested that there must be some anatomical reason for the great ability of the Japanese to digest rice as compared with Europeans. Professor Taguchi, acting on this, measured the intestines of twenty-five cadavers, and now he declares that the European intestine is only about two-thirds as long as is the Japanese.

The Western Opthalmological, Otological, Laryngological and Rhinological Association will meet in St. Louis, Mo., the second Thursday and Friday of April, 1897. Physicians desiring to read papers are invited to send titles to the secretary at once. The profession are cordially invited to attend. Dr. Adolf Alt, president; Dr. Hal Foster, secretary, Kansas City, Mo.; Dr. W. L. Dayton, treasurer.

The Twelfth International Medical Congress will be held at Moscow, August 19-26, 1897.

Claudius H. Mastin, M.D., of Mobile, Ala., has been requested, and has consented to serve as one of the members of the American National Committee of the Twelfth International Medical Congress of 1897.

The Central (Moscow) Executive Committee consists of the following gentlemen: President, Prof. I. F. Klein; vice-president, Prof. A. J. Kojewnikow; treasurer, Prof. N. F. Filatow; secretary general, Prof. W. K. Roth; secretaries, Prof. P. I. Diakonow, Prof. W. A. Tikhomirow, Prof. I. I. Heyding; members, Prof. S. S. Korsakow, Prof. J. F. Ognew, Prof. W. D. Cherwinsky.

It is officially announced that preparations are being made for the reduction of Trans-Atlantic steamer and European railroad fares.

A. Jacobi, M.D., *Chairman American National Committee.*

The meeting of the British Medical Association of Montreal, Canada, on the 31st of August, 1897, will probably interest a large number of American practitioners. But it is important to remember, as we are informed by the Canada Medical Record, that none but members of the association or specially invited guests are allowed to be present at the meetings and to take part in the discussions; not only so, but a recent by-law provides that none but British subjects can gain membership. Therefore, aside from the few Americans who are specially invited to attend, it would hardly be worth the while of many of the remainder who would like to do so to invite gratuitous insult or neglect by appearing at that time in Montreal.

URTICARIA OF THE RESPIRATORY PASSAGES.—Delbrel has collected (Journ. de Méd.) a large number of cases and records of this condition, and from these he draws the following conclusions: There are two types of urticaria affecting the respiratory organs: (1) In certain cases the cutaneous eruption appears first, and is followed by respiratory trouble; (2) in others the respiratory symptoms first appear, to be followed later by the eruption, and it is in this latter that the greatest difficulties of diagnosis and the greatest danger to the patient may arise. In many instances the urticarial affection so closely resembles other respiratory disorders that in the absence of any cutaneous condition diagnosis may be almost impossible, and it may even happen that the only manifestation of the disease is that affecting the respiratory mucous membranes. Some cases simply resemble an attack of asthma; others manifest themselves by suffocative attacks with irritating, hacking cough, closely simulating œdema of the glottis, for which they may be mistaken. In such cases a laryngoscopic examination may be of great use, though, unfortunately, it may fail even in skilled hands, as it seems to cause increase in the symptoms. In the instances where it has been carried out, red, raised erythematous patches have been found in the posterior pharynx, and though it may be impossible to obtain a view of the eruption in the larynx or trachea, the existence of such patches in their neighborhood may be of use. In the cases where the respiratory affection is severe the symptoms may be the most alarming. There is no regularity as to the time of their appearance after the ingestion of some article of diet, etc. The author states that severe cases not infrequently end fatally; others may last for periods varying from a few hours to several days, and the appearance of urticaria seems to be a favorable sign. He suggests that in acute cases with severe pulmonary symptoms and no cutaneous eruption brisk friction should be applied to the skin in order to induce its appearance.—British Medical Journal.

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GAILLARD'S MEDICAL JOURNAL.

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Scientia et Veritas Sine Timore.

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EDITORIALS.

THE ABUSE OF MEDICAL CHARITY.

This subject has received a considerable amount of earnest consideration in England, as well as in this country. In an address on hospital reform, delivered before the Manchester Medico-Ethico Association on November 27, 1896, by T. Garrett Horder, L.R.C.P. of Edinboro, and M.R.C.S. of England, published in the Lancet for December 19, the author wisely observes: "It is perfectly true, as the Lancet recently remarked, that the initiation of any reform must come from the profession; but it is none the less true that the responsibility of carrying any reform into operation must rest with those who are responsible to the public for the proper administration of our hospital system." The author quotes from Mr. Nelson Hardy's paper the three-fold remedy for acknowledged abuses suggested by the Hospital Reform Association as far back as 1872-4, namely; 1st Inquiry into the circumstances of patients by a properly qualified physician; 2nd Stopping the indiscriminate supply of medicine; and 3rd The discontinuance of prescribing by unqualified students. Continuing, the author quotes from Sir William Ferguson on his formation of a subcommittee appointed to investigate this same matter: "The sub-committee have arrived at the conclusion that a very large proportion of the out-patients of general hospitals (variously estimated at from three-fifths to nine-tenths of the whole) consists of trivial cases which do not require any special skill, and might properly be left in the hands of ordinary medical men. An inordinate number of trivial cases wastes the time of the consultee, wearies the attention of the student, and forces a habit of hasty diagnosis and careless observation which tend to irregularities and inefficient treatment. In fact, out-patient work as generally conducted neither conduces to the sound advancement of professional knowledge nor to the advantage either of the students or the public."

The author goes on to say: "For some years past the British Medical

Association has been endeavoring, by means of a special committee, to grapple with this gigantic evil, and it is only just to the members of that committee to acknowledge that their efforts have been attended with some good results." "When it is found that more than one-third of the inhabitants of Dublin, Liverpool, London, Edinboro and Bristol were the recipients of gratuitous medical relief in 1892, it is evident that there is urgent need, not only of investigation but also of radical reform. "Although we know perfectly well that the in-patient department of hospitals is seriously abused, yet it has been brought of late more forcibly before us in the able articles prepared by the special commissioner of the Lancet. I do not intend this evening, however, to deal with the question of the abuse of that department, but shall restrict myself to dealing with what our association considers the most crying evil, namely, the abuse of the out-patient and casualty departments.

"The question of how to remedy the abuse of the out-patient department has been considered by our association, and although we have not yet had an opportunity of finally deciding what remedies we shall propose, we have gathered together the opinions of many medical men, many of whose names will be familiar to you, and some of whom have devoted much time and attention to the subject. The following questions were sent out to these gentlemen: 1st. In your experience do persons well able to pay a doctor's fees avail themselves of the benefit of the out-patient department? 2nd. If you believe that abuse of that department exists, in what way would you propose to remedy it? 3rd. Would you recommend the appointment of competent persons to make inquiries into the circumstances of out-patients? 4th. Would you advise the adoption of a 'wage limit'? 5th. Should there be a limitation of the number of new cases to be seen by each medical officer; if so, what number should be specified? 6th. What method would you suggest for preventing the indiscriminate use of hospital letters?

"To the first question, with one exception, the answers were unanimously in the affirmative. To the second question, which is the most important of the whole lot, there is practical unanimity in recommending a restriction of cases to those sent by some medical authority. Those who do not advocate this view, recommend the abolishing of the department altogether, except for first aid. To the third question there is only one dissentient to the plan of appointing competent persons to make inquiries into the circumstances of patients. To the fourth question the answers are pretty evenly divided between yes and no. To the fifth question there is a pretty general concensus of opinion that the limitation system is desirable. To the sixth and last question there is a large majority in favor of abolishing hospital letters."

We have given extracts from this paper in some detail, because the question is demanding solution in every large city in our country. Like all other questions which become more and more complicated as time goes on, this question can be solved at the outset in a number of the smaller places by the concerted action of a few medical men; but in the large cities where, as a matter of fact, the suffering by medical men themselves from such abuses is greatest, the problem seems exceedingly difficult, although not entirely impossible of solution. The president of the County Society of New York, the largest organization of this kind in New York, gave startling figures taken from the reports of the various dispensaries and out-patient departments of New York city, and, as the result of this paper, a resolution was passed and a committee appointed to immediately enter upon the work of solving this difficult question. The result of their labors will be watched with the deepest concern by the whole profession; as the measures which have already been adopted in Boston, Baltimore, Denver and Washington have not only not proved entirely satisfactory for those places, but have also seemed to be in some of their features not at all applicable to the situation as we find it here. It seems to us that the keynote of the situation lies in the suggestion emphasized by the author of the paper, extracts from which we have quoted at length, namely, that those applying for relief, barring accident and emergency cases, should bring certificates of their financial ability from some reputable medical man.

THE LAY PUBLISHING FIRMS OF MEDICAL JOURNALS IN THE PROFESSION.

We are in receipt of a circular letter from a well-known editor and author, complaining that one of the best known medical publishing firms refuses to permit the editors of a recognized annual work to use in its abstracts of medical progress articles and illustrations first printed in the journals controlled by the publishers mentioned. The claims of the circular letter are as follows:

1. It prevents the dissemination of medical knowledge. The year-book condenses, systematizes, and criticizes the year's medical work in a shorter space and more permanent manner than the journals, and has thousands of readers no single journal can claim, or hope to reach. Every physician writes and publishes articles in order that every member of the profession may, if possible, learn of his work, and that science and progress may thus be furthered and humanity benefited. To interfere with such dissemination of our literature in reputable publications is, I think, discourteous and unjust to the profession and an injury to medical science.

2. This injustice and injury to medicine become all the more striking when physicians do not receive a cent of pay for contributions, from the publication of which the lay-publisher is supposed to make considerable financial profit.

3. No other publishers in the world, not even those who pay authors for their contributions, have in the least objected to our reproduction of quotations, abstracts, and illustrations from their journals.

We are quite in sympathy with these claims, and we are entirely in accord with the supposition that the literature for which no compensation is received belongs to the profession and not to the publisher. Indeed, we have always understood that even the journals which pretended to pay, either in cash or reprints, for accepted articles, considered their use, whether as abstracts or as selections in other medical journals, a tribute not only to the intrinsic worth of the article itself but also to the ability and good judgment of the editors of the journal in which it originally appeared—all provided, of course, that proper credit was given. The benefits that can by any possibility accrue to the authors of the articles in the various medical journals are assuredly sufficiently meager to forfend the exclusive use and ownership of such article by any single journal. We cannot believe that the publishers of whom complaint is made will find it to their interest to persist in such a suicidal course; but if they do, the profession has in its own hands a rapid and satisfactory solution of the matter, namely, to insist when the article is offered for publication that the right of abstracting the text or of reproducing illustrations is guaranteed.

THE SERUM DIAGNOSIS AND TREATMENT—PROPHYLACTIC AND CURATIVE—OF TYPHOID FEVER.

We have given in this month's issue one or two of the best practical references to the subject stated in our caption, and do not propose at the present time and place to consider these matters in detail; but we do wish to emphasize what seems to be a most valuable addition to our diagnostic methods, and what seems to promise considerable success in the prevention as well as the cure of one of our most dreaded diseases. When the diagnosis by the Widal method is placed upon an absolutely sure footing, it will have been a great step toward the solution of some of the most difficult cases presented to the medical men; not only so, but it will materially increase for the patient the chances of recovery, by enabling the physician to intelligently combat almost at its outset a disease which has frequently been impossible of diagnosis for a considerable period. The prompt action of the New York Board of Health in the matter, as well as all the boards in some of the other large cities of this country, speaks with no mistaken meaning in regard to the value attached by this organization to the worth of the discovery. We shall have occasion to follow the matter up possibly in several issues of the Journal, and while the prophylactic and curative sides of the question may not be developed for a considerable period, the activity attached to the diagnostic aspect will assuredly result in, we hope, practical and definite measures available to the whole profession.

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ORIGINAL ARTICLES.

DIASTASE IN THERAPEUTICS.

By C. C. FITE, M.D., New York.

Read before the New York Medico-Surgical Society, January 4, 1897.

Abstract of the discussion engaged in by W. L. Stowell, M.D., W. A. Walker, M.D., George Tucker Harrison, M.D., Henry Dwight Chapin, M.D., Morris Manges, M.D., Wm. Henry Porter, M.D., Charles Henry Brown, M.D., Glover C. Arnold, M.D., John Blake White, M.D.

The sprouting of a seed was considered, until quite recent times, a great mystery; not until diastase was discovered could we properly understand why a seed after being placed in a moist soil began to develop into a plant after having lain dormant perhaps for years. Under proper conditions, the life-endowed germ is no longer a sleeping unknown quantity, but it is an active principle and the process of development begins.

The sprouting of seed is a very interesting subject for scientific study—this awakening of the germ life, by the influence of heat and moisture, and the action of diastase in converting the starch of the seed into maltose, which afterwards becomes fibrous or cellular tissue. We will not go into a discussion here in reference to the secretion by the plant of fluids which, acting through the delicate roots, absorb and utilize the soil elements; of the important work done by the leaves in taking carbon from the atmosphere; and by these and other processes completing the growth of the plant and eventually a reproduction of seed to continue the life of the species indefinitely.

Let us now leave the plant and refer to the important factor in the growth and development of animal life; the ptyalin of the saliva, a product analogous in many respects to diastase. Just as Nature places diastase in the grain to produce the changes leading up to a higher growth, so she gives animals ptyalin to convert the starch and perhaps other foods into assimilable material for nourishment, for heat, and a reserve supply of fat. I use the words nourishment, heat and fat, advisedly. The starch so converted is nourishment, and it is the basis of our coleric energy. Our supply of fat comes mainly from the starch changed into maltose by the ptyalin, supplemented by the pancreatic secretions, and it is changed into oil later on in the process. This oil is not only utilized for heat but stored in the tissues as a reserve supply.

It is well to bear in mind that few carnivorous animals take on a great degree of fat; grain eating animals do. Give swine all of the fat and oils they can eat, and they will not gain in weight half as quickly as when fed on grain alone.

The function of ptyalin is to convert starch into dextrin and maltose, this being the preliminary step, and goes on in the normal stomach for from thirty to forty minutes, after the close of an ordinary meal, when the acid peptic digestion stops the diastasic process. The duodenum holding the pancreatic and other secretions takes up the partially changed starch and completes the conversion. We need not follow the process beyond this point.

It has long been observed that we do not get altogether satisfactory results in the treatment of amylaceous indigestion with pancreatic extracts. This is probably due to the fact that the amylopsin of the pancreatic juice and the other various duodenal enzymes are intended more for completing and finishing the changes already begun by ptyalin before the peptic digestion supervenes, and are not adapted for this preliminary conversion in the stomach. In other words, they are not suitable for beginning the conversion in the food mass as found in the stomach—acting we might say on the mass later when it is an acid body, during the time it is being changed from an acid to an alkaline reaction, instead of on the alkaline or neutral mass when it is being changed into an acid mass. We see, therefore, that while ptyalin and amylopsin are practically identical in their action on starches, Nature intends them to act under different conditions and at different periods of the digestive process. Therefore, we should not give pancreatic extracts with the expectation that they would render desirable service in the stomach, but we should rely upon either increasing the supply of ptyalin by slowly masticating the food, or by giving a ferment having similar properties to ptyalin; this we have in diastase.

From the discovery of diastase in malt, until quite recently, many attempts have been made to produce it in an isolated form economically enough to be used freely in medicine; without having to give it in the form of the semi-solid malt extracts which have been the only reliable form of it readily obtainable by the profession, as the liquid malt extracts do not contain an appreciable amount of diastase.* The disadvantage in its use in malt extracts is that we have to give a large bulk of the extract to get a very small quantity of diastase; then too, the extract contains fermentable sugars and extractive matters which may not only be of no value, but may give rise to fermentation in the stomach or intestinal tract.

So far the only chemist, who has succeeded in producing an isolated diastase economically and on a large scale, is Jokichi Takamine, the Japanese chemist. (See the London Lancet, May 25, 1896.) This investigator received his scientific education at the Glasgow University. He devoted some years to the study of malting and the production of diastase and other ferments, and upon his return to Tokio was fortunate enough to find that Eurotium oryzae was what he desired. The process he finally perfected is in brief as follows: The seed of the Eurotium is sown on moistened and sterilized wheat bran. The growth is rapid, and after the plant has reached maturity, he calls the bran with the growth on it "Taka-Koji." Upon examining the growth, under the microscope, it is found that the roots of the Eurotium which have penetrated the bran, are covered with crystals of pure diastase. These diastase crystals have the property of converting the starch of the bran for the nourishment of the plant. Takamine named the diastase so produced, Taka-diastase. In preparing this diastase for use in medicine it is necessary to get rid of the spore. This is done by percolating the Taka--Koji with water, and adding to this solution of diastase, alcohol, when the diastase is precipitated and the activity of the spores destroyed. It is then a simple matter to further purify the diastase and preserve it in a dry powdered form indefinitely.

This diastase, owing I presume to it being an isolated substance, acts much quicker than the diastase found in malt extracts. It will convert 100 times its weight of starch in ten minutes under proper conditions, if the process is continued for three hours, 1,500 times its weight. It is therefore evident that its amylolytic-converting power is quite marked, and the theoretical position that it would prove of value in the treatment of amylaceous indigestion has been verified by careful observers many times over.

Prof. Leo, of Bonn, claims that this diastase exerts its action in a higher degree of acidity than was first claimed by those of us who had experimented with it, and that it is, therefore, superior to ptyalin in its starch-converting power. He has employed it with benefit in cases of deficient salivary secretion, as also in hyperacidity of the stomach.

Dr. W. S. Christopher, of Chicago, (Therapeutic Gazette, March, 1896), holds that flatulence is due to micro-organisms which attack unchanged starch and give rise to fermentation, and that it is therefore important in these cases to predigest the starch in the stomach, and the more complete this process, the less food there is for the micro-organisms to act upon in the duodenum. He finds that the administration of diastase in these cases gives satisfactory results.

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*See The Boston Medical and Surgical Journal for Dec. 31, 1896, page 669.

Dr. R. W. Wilcox, of this city, has given very close attention to the therapeutics of this question, as shown by two papers he read some months ago. One before the N. Y. State Medical Society, January, 1896, and one before the N. Y. Academy of Medicine, February 18, 1896. Both papers being published in the Medical News.

Other favorable clinical reports which have attracted my attention which may be mentioned here are—in the Journal of the American Medical Association for August 15, 1896, by Dr. T. H. Allen; in the Medical Age for July 25, 1896, by Dr. F. Spencer Halsey; and in the Therapeutic Gazette for September 15, 1896, by Dr. William A. Walker.

In the practice of some friends, who have reported the matter to me for elucidation, this form of diastase seems to have other properties than its action on starch. In one notable case in which pepsin and other methods of treatment failed to give any benefit, although every symptom and test seemed to indicate that it was an undoubted case of albuminous indigestion, the result was not only palliative but curative. I have endeavored to have this matter settled by laboratory experiments, but so far the results have not been entirely conclusive, so any theory on this question for the present must be based mainly upon clinical evidence. This diastase does, as was shown by the experiments referred to, disintegrate albumens, but the proteolytic action apparently stops short of the production of albumoses and peptones. As to what takes place in the intestinal tract, of course we cannot yet say. It is altogether probable that the benefit in these cases is due to the promptness with which the first period of digestion is carried on, and the conversion, instead of the fermentation of the starchy foods, leaving the second process, the acid peptic digestion, to go on normally without being interfered with by deleterious products, and the partial disintegration above referred to doubtless promotes the activity of the gastric juice by giving it freer access to the particles of albumen.*

We should always bear in mind that we have from thirty to forty minutes after the close of an ordinary meal in which the action of ptyalin or diastase will continue before the acidity of the stomach contents reaches the point at which such converting power is impaired or destroyed. The proper theory for the administration of diastase is that it supplements the ptyalin of the saliva, and the more thorough the preliminary digestion in the stomach, the less work there is to be done in the duodenum. The formerly prevalent theory that pancreatic extracts and diastase ought in some mysterious way to find their way through the stomach into the duodenum, and there begin their work, is too absurd to be entertained. (See Dr. Walker's article above referred to.) I am indebted to Dr. Henry Dwight Chapin for information in this connection (N. Y. Medical Journal, September 16, 1893), the results of some elaborate experiments which he had made in the Post-Graduate Laboratory in 1893. The experiments were made with a product containing diastase; the stomach was washed out and after the subject had been properly fed the diastase was administered in certain cases and omitted in others. The report made to Dr. Chapin by the chemist in charge of the work was, that, when the stomach was emptied forty minutes after the administration, the percentage of food remaining in the stomach at this time averaged, when diastase was not given, 52.02 per cent.; with diastase, 29.2 per cent. This showed very conclusively the action of the diastase in the stomach. An analysis of the solid food remaining in the stomach showed that when diastase had not been administered 7.02 per cent. were undissolved; when it was given, only 3.45 per cent.

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* Since this paper was written, I have been informed by Mr. F. A. Thompson, a skillful Detroit chemist who has given close attention to this subject and performed a number of elaborate experiments with diastase and other digestive ferments, that he has found that Taka-diastase does undoubtedly, not only disintegrate albumens, but produces albumoses and peptones as well. This is an important and interesting discovery, and perhaps confirms the work of the German chemist, Bezanes, who found that in producing diastase from malt, a by-product which he named peptase was formed, which acts on albumen. It is therefore evident that as there is formed in grain a product to change the albumen of the grain as well as the starch-converting ferment, so the Eurotium has a ferment similar to peptase for converting vegetable albumen for its nourishment. The peptase produced in malting is extremely slow in its action, due perhaps to its being interfered with by the thick, gummy malt products.

Experiments with diastase have not up to this time been as satisfactorily conducted as those made with pepsin, for the reason that diastase is a sensitive body, and the value of any laboratory or test-tube experiments are apt to be contradictory, unless proper precautions are preserved as to the degree of heat used, and the various brands of starch found in the market vary in reaction and in their sensible properties. It is to be hoped that we will soon have definite tables in use for this work, so that all experiments being made by a uniform standard the results can be more intelligently compared and studied.

I might add in conclusion that the therapeutic properties of diastase have not yet been as thoroughly investigated as could be desired. A good deal of careful work is now being done by investigators in this and other countries, and I am confident that the literature of the subject will be enriched very greatly in the near future.

So far little has been done with diastase in practical medicine beyond its use in typical cases of amylaceous indigestion. What we may expect from its employment in partially converting the starch of barley water for infant feeding remains yet to be determined, but I am very hopeful of its use in such cases. The fact of the casein of cow's milk being so much more dense and liable to form tough curds than human milk, has led me to hope that if we partially convert the starch of the barley water it will become not only a mechanical diluent for milk, but a readily absorbable food as well, which will nourish and not give rise to fermentation and flatulence.*

Some time since I witnessed several experiments in bread-making made by Mr. C. Von Egloffstein, then of Yonkers, now of Brooklyn, N. Y., to determine the value of diastase in rendering bread more soluble. His conclusions were that the soluble matter in ordinary bread in water at a temperature of 100° F. represented at the end of an hour 15 per cent. whereas when a proper amount of a product containing diastase was added to the bread before baking the soluble matter under the same conditions was 40 per cent.

It is more than probable that diastase will play an important part in the therapeutics of the future.

RESIDUAL URINE OF THE URETHRA.+

By STUART McGUIRE, M.D., Richmond, Va.,

Professor of Principles of Surgery in the University College of Medicine, and Surgeon to St. Luke's Home, The Virginia Hospital, and the Home for Incurables.

The penis is both a urinary and sexual organ, and, like all compromises, has certain defects which render it liable to disease. Disorders of the kidney and bladder affect its sexual function, and venereal troubles cripple it for the discharge of its urinary duty. So intimately are the two associated, that in treating disease of the one the possible influence of a pathological condition of the other must be constantly considered.

There is no condition met with in genito-urinary practice so difficult to cure as chronic posterior urethritis. I do not propose to discuss the disease systematically in this paper, but I want to call attention to the fact that in many cases the condition is maintained and the treatment frustrated by the presence of residual urine in the urethra. It has long been known that a few drops of urine may be retained behind a tight stricture, but I have been unable to find mention of the possibility of a considerable quantity of urine being left in a dilated portion of the urethra, and acting as a causative factor, or as an obstacle to the cure of the disease.

The urethra is not a tube of uniform caliber but has points of physiological narrowing. It is divided arbitrarily by anatomists into three portions—the spongy, the membranous, and the prostatic; by surgeons into two—the pars anterior and the pars posterior. The urethra has two curves

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* See Dr. Chapin's remarks in the discussion following.

+ Read before Richmond Academy of Medicine and Surgery, December 8, 1896.

—one fixed by the prostate, the other movable and depending on the position of the penis. When urine passes through the urethra it is propelled not only by the *vis a tergo* of the bladder, but by the contraction of the various muscles, and the channel is normally emptied of the last few drops of fluid by a progressive wave of blood which flows from the bulb through the corpus spongiosum. A careful study of the anatomy of the urethra will at once suggest the possibility of urine stagnating at certain points; and a review of the physiology of micturition will show how nature has seemed to foresee the evil results which would follow, and guard against its occurrence.

It was my misfortune at the very beginning of my professional life to have several patients with chronic inflammation of the deep urethra. I treated them with indifferent success, and, from my inability to cure them, attributed the symptoms of which they continued to complain, to sexual neurosis or hypochondriasis. Finally, I bought an electric urethroscope, and began to examine systematically every case of chronic urethral trouble that came into my office. At first, I accomplished little; but after I became familiar with the healthy and the diseased appearance of the mucous membrane of the canal, and learned by experience what local applications did most good, my results were very gratifying.

In several different cases, when I looked down the tube, I almost invariably found its end filled with fluid, and so constant was this condition that I expected to find it, and had a mop ready to remove it. I did not at the time appreciate its significance, and supposed that I had introduced the instrument too far, and had dilated the sphincter of the bladder. Last winter a patient came to see me, suffering with chronic posterior urethritis, but, in addition to the usual symptoms, he said that after urinating his trouble was greatly increased, and that he could only obtain relief by stroking his perineum firmly with his finger from behind forwards, and thus milking out about half an ounce of urine which remained in the deep portions of the urethra. The passage of a No. 30 sound failed to detect a stricture, and I was forced to the conclusion that the residual urine was not dammed back by an obstruction, but was retained in a dilated and inelastic pouch of the urethra. I have now the record of four other similar cases, the quantity of retained urine varying from one drachm to half an ounce.

Residual urine of the urethra may be caused in one of two ways or by a combination of both. Either there may be a stricture of the urethra, and the urine rushing down from the bladder meets with the obstruction, and by hydrostatic laws expands the portion of the canal behind the stricture, and the repeated distention causes the part to lose its elasticity and contractility and remain patent; or there may be no stricture; but a chronic inflammation of the mucous membrane and adjacent structures may so lessen its tone and relax its tissues that dilatation and sacculation follow. In both cases the result is the same; urine is retained in the urethra, and, undergoing decomposition, irritates its sensitive surface and produces distressing symptoms.

It is a question whether residual urine in the urethra is the cause or the consequence of posterior urethritis. The practical fact is that the condition cannot be cured till it be removed.

The treatment of such cases must be moral, hygienic, constitutional, and local. The patient is in a state of mental depression bordering on sexual neurasthenia. He is as morbid and hysterical as a woman with "womb disease." By kindly sympathy and judicious encouragement, the surgeon should win his confidence and overcome his fears. The patient's diet should be restricted, his bowels regulated, and a moderate amount of exercise advised. If he be married, sexual intercourse need not be interdicted; but if he be single, he should remain continent and carefully avoid all possible sources of excitement. Tonics are frequently useful; if the patient be weak and has no appetite, give him a bitter stomachic like tincture of cinchona; if he be pale and anæmic, give him large doses of tincture of the chloride of iron. Direct him to "strip" his urethra after emptying his bladder; and if his urine is concentrated or irritating, instruct him to drink large quantities of some pure light water. Salol, or some other drug which is eliminated by the urine and by its antiseptic properties prevents its decomposition, may be frequently used with benefit.

The local treatment is of great importance. The first point to be determined is the presence or absence of stricture. If it be present, it should be dilated by the systematic use of large sounds. If it be absent, or if the symptoms continue after it has been removed, the case should be treated by making stimulating applications directly to the diseased area. The whole length of the urethra should be rigidly inspected with the urethroscope, and the congested spots, granular patches, or superficial ulcers, accurately located and carefully touched with a solution of nitrate of silver, the strength being varied to suit the requirements of the individual case.

Before the developments of urethroscopy, urethral lesions were unrelieved because unrecognized. We live in an age of accuracy and precision, and with modern instruments have no excuse for empiric practice.

SELECTIONS.

LECTURES ON ANGINA PECTORIS AND ALLIED STATES.

By WILLIAM OSLER, M.D.,

Professor of Medicine, Johns Hopkins University, Baltimore.

Lecture VII.—Diagnosis, Prognosis, and Treatment of Angina Pectoris.

Treatment.—"The first and great object of the practitioner on being called on to treat a case of angina will be to make himself acquainted with its individual character. Beginning with the early history of the disease, he will trace it to its present stage, and will endeavor, from the narrative of the patient and from the observation of the whole phenomena presented to him, to form a clear judgment respecting the local condition of the organs in which the characteristic symptoms have their site, and the state of all the other parts of the system which can in any way influence these; in other words, he must endeavor to ascertain the species or variety of angina, according to the distinction formerly pointed out." This clear statement of Sir John Forbes forms a fitting introduction to the discussion of this part of our subject. Successful treatment depends often upon correct diagnosis; but there are cases of angina pectoris brought to the consultant in which diagnosis and prognosis in themselves constitute the treatment. To a man who has felt that judgment has been given against him, the doom pronounced, and whose mind is haunted with the dread of sudden death, the assurance that the condition is functional and curable comes as a reprieve, and may be the one thing necessary to effect a cure.

True Angina.—Determine in the first place, if possible, the existence of any constitutional disease, as syphilis, gout, or diabetes, and the presence or absence of valvular lesions.

(a) General Management.—Inquire carefully about the exciting causes of the attacks, which differ in different cases. Usually the patient has learned by bitter experience his limitations in certain directions, and knows much better than you can tell him just what to avoid; but you can emphasize the importance of mental worry, exercise, and diet, the three chief factors. Quiet of mind, avoidance of worries and cares, the cultivation of a calm equanimity—with these, or such like phrases, we try to impress a poor victim who, to previous anxieties has now the added burden of a disease, the terrible character of which he can appreciate but cannot understand. Our words often seem a mockery, and yet they may be helpful in persuading a man to cast off all unnecessary business and to live a life in which there shall be a minimum of friction. Time, too, with its soothing deception, comes to allay the access of early apprehension, and as succeeding attacks pass there may be less and less mental distress. An important question arises here. Shall a man with angina give up his business? In a majority of cases this sacrifice is unnecessary; the literature abounds with examples of men who, like John Hunter, have done the best work of their lives after the onset of angina. There is so much uncertainty that no rule can be laid down; each individual case must be considered separately. The patient's age, occupation, and, above all, the condition of the vascular system, must be taken into account. Even after a most severe attack, followed by a cardiac breakdown of several months' duration, a man may be able to resume work, and, as in Case V, referred to in Lecture III, be benefited by the steady occupation.

Exercise must be taken within the limits which each individual soon learns to recognize. In severe recurring attacks, induced by slight muscular efforts, a period of absolute rest should be enjoined. The sudden, quick movements which rapidly increase the blood pressure and throw a strain upon the heart are the most dangerous: and most of all those with which are associated strong emotions. The patient should be urged to walk on the level, in the literal as well as metaphorical meaning of the phrase. He should learn "to live within the income of his circulation," with which wise saw from the lips of the late Dr Sibson a friend with organic heart disease has been comforted and sustained for a quarter of a century. Steady, quiet exercise should be encouraged, except, of course, when there are special signs of cardiac weakness, in which case the resistance gymnastics of the Schott method may be tried.

Diet is in many cases the central point in the treatment. The subjects of angina are often men with large appetites, accustomed to eat freely of rich and strong foods. First, limit the amount taken, which in most persons above forty years of age is far too great; second, see that the quality is suitable by excluding from the dietary rich, highly seasoned foods and those which favor fermentation; and third, arrange the hours for eating. The subjects of angina are usually aware of the necessity of limiting the quantity of food and drink taken at one time. So soon as the stomach is distended there may be warnings of distress about the heart, or in aggravated cases a full meal may always cause an attack. As one patient expressed it, "Had I not to eat, I would never suffer." Light meals should be the rule in all cases; at breakfast and at midday dinner more may be taken than at the evening meal. Late suppers should be interdicted—there is "death in the pot" for angina victims, and a surfeit may be as fatal as poison.

The quality of the food is equally important. Special dietaries may be necessary for patients with gout and glycosuria, but in ordinary cases the food is to be regulated with reference to one all-important feature—viz., flatulence. As you may remember, almost every one of the old writers laid the greatest stress upon this element in the causation of the attacks, and they were right. In dealing with the question of diet we are too apt to adopt some fad to which, with Procrustean precision, we fit every case. A more rational way is to recognize the extraordinary peptic diversity in our patients—in no respect more strikingly shown than in this very matter of flatulency. Beyond the generally accepted restriction of the carbohydrates we cannot go very far without meeting individual peculiarities which have to be considered. The patient himself has to be consulted carefully. Some of you may call to mind what our distinguished colleague Dr. Smollett makes one of his characters, Matt. Bramble, say in Humphrey Clinker: "For my own part, I have had a hospital these fourteen years within myself, and studied my own case with the most painful attention, consequently may be supposed to know something of the matter." We are too apt to forget this. An intelligent man should be able to tell you just what articles of food cause most disturbance and produce wind in the stomach or bowels. The fault may not lie in the food, but in the inability of the stomach and bowels to digest it properly. The obese, flabby subjects of angina—not the most numerous class in my experience—and those with weak heart and anteriosclerosis are specially prone to flatulence. A few doses of blue mass, an occasional saline purge, and the use at times of a good bitter tonic keep this condition in check. The use of hot water before meals, particularly before breakfast, has been found very serviceable.

In elderly men accustomed to stimulants, hot grog at bedtime allays the tendency to flatulency, which is sometimes the cause of wakefulness, or which is apt to disturb the patient in the early morning hours. Peppermint, spirits of camphor, Hoffmann's anodyne, carbolic acid, iodine, and creosote are useful for "wind on the stomach." For the intestinal flatulency a saline purge is often a good corrective; the various supposed intestinal disinfectants may be tried—salol, beta-naphthol, and corrosive sublimate, of which pilules of from one-sixtieth to one-thirtieth of a grain may be given sometimes with advantage.

(b) General Medical Treatment.—Of constitutional conditions underlying angina pectoris and capable of treatment, syphilis and gout are the most important. Genuine angina in a man under thirty-five years of age should arouse a suspicion of syphilis, and vigorous measures should be adopted. In gouty cases free elimination by the bowels, skin, and kidneys should be secured, a proper diet ordered, and at intervals a course of colchicum may be prescribed.

One patient, Dr. ———, emphasized repeatedly the benefit he had derived from colchicum. Stimulants should be avoided. Glycosuria is usually controlled by diet, and rarely gives much trouble.

In a large proportion of all cases of angina pectoris the treatment consists in the administration of the iodides and nitrites, remedies which are believed to influence arterial function and arterial nutrition. The use of iodides of potassium and sodium in this disorder has been advocated most warmly by Huchard, who states that of eighty patients with organic angina treated thoroughly by these drugs twenty-two recovered, forty-three were greatly benefited, and fifteen died.* The iodides appear to have a beneficial effect in checking or modifying the progress of arteriosclerosis and in lowering the blood pressure. They may influence, too, arterial pain. I have called your attention repeatedly to the influence of iodide of potassium in aneurism of the aorta, in which the relief of the pain is one of its most striking effects. While I cannot say that my experience is in every way so favorable as Huchard's, I can testify to the great relief which has followed its use in many cases, and in a few an apparent cure. Cases which were thoroughly treated nearly ten years ago remain quite well, and I have had within the past three years several patients who have been greatly benefited. I usually order the iodide of potassium in doses of ten or fifteen grains three times a day. Should it disagree, which is very seldom, I give the sodium salt. Larger doses are not often necessary. If intolerance develops, stop the use for a week and begin with smaller doses. The success in treatment depends upon the perseverance with which the drug is used. On this point let me quote from Huchard's pamphlet: "One of the principal conditions of success is perseverance—constancy in the medication. The drug must be taken for a period of two to four years, in daily doses of one to three grammes, until all symptoms of angina have disappeared for many months, and I hold that a permanent and definitive recovery is not obtainable except after many years of treatment." Reasonable caution must be employed, and you would not give the iodides in patients with advanced arterial degeneration, a dilated heart, and signs of interstitial nephritis. The patients who stand the treatment well are the robust, middle-aged men in whom the angina is the sole symptom. With aortic disease, if fairly compensated, the drug may be used.

The nitrites in hypertension and angina pectoris are of value quite equal to the iodides. The nitrite of amyl is employed in the paroxysm. The nitroglycerine or trinitrin is indicated in all cases in which the tension is persistently high. Given properly, it is a very valuable remedy, but to get any advantage from its use each case must be taken by itself. In the first place, be sure that the nitroglycerine, either in solution or tablets, is fresh. The tablets containing one one-hundredth of a grain are, as a rule, reliable. It is well to begin with only one of these three times a day. The dose may be increased gradually until the patient takes four or five three times a day, or even a larger dose. If the patient notices a slight glow or flush and a little sensation of fulness in the head you may know that the remedy is acting. I feel sure that in individual cases we often do not employ the drug in sufficient doses. I have never seen it do any harm. The extreme flushing and throbbing headache give reliable indications when the limit has been reached. I have given as much as thirty minims of the one per cent. solution, three times a day, to a case of chronic arteriosclerosis, without any disturbance. The nitrite of sodium, recommended by Hay, may also be tried in doses of five to ten grains three times a day.

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* Le Traitement de l'angine de poitrine, Paris, 1892.

Among other remedies which are useful in the general medical treatment of angina, arsenic is sometimes very valuable. Balfour advises it particularly in the weak heart of elderly people, when associated with pain of any kind. In cases of feeble heart with anæmia, iron and strychnine are most valuable remedies, and in order not to trouble the patient with too many doses, arsenic, iron, and strychnine may be given together in compressed powder or pills.

(c) Treatment of the Attack.—So frequently is the paroxysm excited by gastro-intestinal disturbances that the subjects of angina should not only be warned to be on their guard in this matter, but should be prepared to take prompt measures on the first indication of any distress. No doubt it was from this standpoint that W. W. Ord made the somewhat paradoxical remark that if restricted to the use of one drug in angina he would prefer sulphate of magnesium to nitrite of amyl. A patient should be told to use a saline purge or blue mass or small doses of calomel when he feels gastro-intestinal uneasiness. It frequently happens that much more prompt treatment is necessary for a condition of flatulency. He should be provided with Hoffmann's anodyne and spirits of camphor; a teaspoonful of each in some peppermint water or hot whisky makes an excellent carminative draught. The combination of morphine, cannabis indica, hyoscyamus, capsicum, peppermint, and spirits of chloroform which is now prepared either in liquid or tablet form as chlorodyne is sometimes very advantageous. In tablet form it is particularly convenient, as it may be carried in the waistcoat pocket.

For the paroxysm itself there are three remedies:

Nitrite of amyl, two to five minims, inhaled from a handkerchief, or from cotton wool placed at the bottom of a wineglass, gives prompt relief in certain cases. The patients are in the habit of carrying the remedy in perles containing three to five minims, which can be rapidly broken in a handkerchief and inhaled as soon as the very earliest symptoms of the attack are noticed. The introduction of this drug, in the treatment of angina, by Dr. Lauder Brunton has certainly been a great boon to many sufferers, but too much must not be expected of it. It is singularly uncertain. While in one case the attacks are promptly cut short and almost immediate relief obtained, in others it seems quite inert. Curiously enough, considering that its physiological effect is in dilating the peripheral vessels and relieving the widespread angeiospasm, in my experience it has been less efficacious in the vasomotor type of the disease than in cases of organic angina. It may produce its effect with great rapidity, as shown by the flushed face of the patient and the increased volume and softness of the pulse, without relieving the pain. It sometimes acts better, given by the mouth, combined with the tincture of capsicum in peppermint water.

Morphine hypodermically is the most useful drug in the attack, and if the pain is not relieved quickly by the nitrite of amyl an injection of a quarter of a grain should be given, and repeated in a half or three-quarters of an hour if the patient is not relieved. In one case the nitrite of amyl failed repeatedly to give the slightest relief, but from a quarter to a third of a grain of morphine, hypodermically, never failed to allay the terrible distress, and seemed also to steady and improve the heart's action A point about the use of morphine in angina which I have never seen mentioned except in the paper by Dr. Burney Yeo in the Practitioner, already referred to, is the remarkable tolerance of morphine in certain cases. In reporting Case XXXII I mentioned that this patient received between ten o'clock on Saturday night and 1 P. M. on Sunday five grains of morphine hypodermically and by the mouth, which relieved the pain but did not give him sleep. There are cases in which a hypodermic injection of a quarter of a grain of morphine given at the first indication of the attack, as a numbness in the hand or tingling in the fingers, checks it at once.

And third, in any paroxysm of great intensity, while waiting for the nitrite of amyl or morphine to take effect, chloroform may be dropped upon a handkerchief and inhaled. Balfour recommends that it be poured on a sponge in a smelling bottle, and the patient told to breathe it through the nose as deeply as possible. In a minute or two relief is obtained, and as the patient comes under the influence of the drug the bottle drops from his hand, and there is in this way no danger of an overdose. The chloroform acts much more promptly and is much pleasanter to take than ether, and I have never seen any dangerous effects from its use, even in persons with very weak heart's action.

(d) Treatment of the Complications.—For the syncope of serious attacks the aromatic spirits of ammonia with Hoffmann's anodyne and brandy may be given, or hypodermic injections of ether or camphor. For the dilatation of the heart and cardiac weakness, which sometimes follow the attack, the nitroglycerine with strong frictions to the limbs may favor the circulation at the periphery, while digitalis or digitaline may be given freely to stimulate the heart's action. Digitaline sometimes acts well, as in Case XXXVIII, and may be given hypodermically. No hard-and-fast rule can be laid down regarding the use of digitalis. It sometimes acts badly, as in a case very carefully studied by W. T. Sharpless, of West Chester. Caffeine and camphor may also be employed. If all these measures seem futile, I would not hesitate to employ puncture of the heart—cardiocentesis—which may arouse to quite vigorous action a dilated and paretic organ. I do not know that this has been employed in the cardiac asystole following a severe paroxysm of angina, but there are instances on record notably the case of Sloane (Edinburgh Medical Journal, vol. xl), in which puncture of the heart with a needle driven firmly into the ventricle has aroused the flagging action without apparently doing the slightest injury.

For the condition of chronic *etat de mal angineux*, in which, for a period of many days or even weeks, the patient has recurring attacks with cardiac asthma and feebleness of the circulation, your resources will be taxed to the uttermost. For the dyspnœa and the Cheyne-Stokes breathing full doses of strychnine, hypodermically, may be employed, from a fortieth to a twentieth of a grain, three or four times a day. Special care should be taken that the bowels are kept freely opened. The cardiac measures already spoken of may be employed, and flying blisters to the præcordia and to the bases of the lungs may sometimes give relief.

Treatment of Pseudo-angina Pectoris.—The measures must usually be directed to combating the underlying condition of neurasthenia or hysteria. Occasionaly it happens, particularly in medical men, that the mental relief afforded by a positive diagnosis of pseudo-angina is in itself sufficient to effect a cure. Cases II and III given in Lecture V, are good illustrations of the improvement and permanent cure, up to the present date, of attacks of maximum severity. It is not easy to say to what the rapid relief could be attributed, as the patients were given only general tonics. In other cases the attacks recur for years, as in the wife of the physician from the Province of Quebec, of whom I spoke, who had had attacks for twenty-five or thirty years. In the severe form, particularly when associated with much vasomotor disturbance, the Weir-Mitchell treatment may be tried with advantage. The effects of seclusion, systematic massage, and electricity, particularly the static form, are sometimes most satisfactory. Where this is not feasible hydrotherapy should be tried, either a systematic course at some institution, or, if this is not practicable, the systematic use of the wet pack at night, followed by thorough friction, will be found advantageous. Some of these cases, particularly if treated at the patient's home, tax to the uttermost the resources of the physician. The change of air and scene in traveling will often be found of advantage.

Drugs are of uncertain and doubtful benefit. We often have to order the bromides and valerian, and in cases with much cardiac irritability and vasomotor disturbance the use of nitroglycerine in large doses seems sometimes to aid in equalizing and steadying the circulation. In looking over the notes of my cases of pseudo-angina I notice this hopeful feature, that with but one or two exceptions the patients are at present not only alive and well, but free from attacks.

When the attacks of angina are due to the abuse of tobacco, the patient should give up the habit entirely. I do not think there is much risk, either, in stopping abruptly. Counter-irritation over the heart by means of the Paquelin cautery or blisters, the use of strychnine in full doses, and, if the pulse tension is high, that of nitroglycerine, are measures which will be found efficacious.

In the worry and strain of modern life arterial degeneration is not only very common, but develops often at a relatively early age. For this I believe that the high pressure at which men live, and the habit of working the machine to its maximum capacity, are responsible, rather than excesses in eating and drinking, or than any special prevalence of syphilis. Angeiosclerosis, creeping on slowly but surely, "with no pace perceived," is the Nemesis through which Nature exacts retributive justice for the transgression of her laws—coming to one as an apoplexy, to another as an early Bright's disease, to a third as an aneurism, and to a fourth as angina pectoris, too often slitting "the thin spun life" in the fifth decade, at the very time when success seems assured. Nowhere do we see such an element of tragic sadness as in many of these cases. A man who has early risen and late taken rest, who has eaten the bread of carefulness, striving for success in commercial, professional, or political life, after twenty-five or thirty years of incessant toil reaches the point where he can say, perhaps with just satisfaction, "Soul, thou hast much goods laid up for many years: take thine ease," all unconscious that the fell sergeant has already issued the warrant. How true to life is Hawthorne in the House of the Seven Gables! To Judge Pyncheon, who had experienced a mere dimness of sight and a throbbing at the heart—nothing more—and in whose grasp was the meed for which he had "fought and toiled and climbed and crept;" to him, as he sat in the old oaken chair of his grandfathers, thinking of the crowning success of his life, so near at hand, the avenger came through the arteries.

"With what strife and pains we come into the world we know not, but it is commonly no easy matter to get out of it," Sir Thomas Browne says; and, having regard to the uncertainties of the last stage of all, the average man will be of Cæsar's opinion, who, when questioned at his last dinner party as to the most preferable mode of death, replied—"That which is the most sudden." Against this, one in a string of grievous calamities, we pray in the Litany, though De Quincy insists that the meaning here is "unprepared." In this sense sudden death is rare in angina pectoris, since the end comes but seldom in the first paroxysm. Terrible as are some of these incidental conditions accompanying coronary artery lesions, there is a sort of kindly compensation, as in no other local disease do we so often see the ideal death—death like birth, "a sleep and a forgetting."

THE KLEBS-LÖFFLER BACILLUS IN APPARENTLY NORMAL THROATS AND NOSES.

By H. W. GROSS, M.D.,

Former Bacteriologist to the Children's Hospital, Boston.

Though it be generally admitted that the chief etiological factor in true diphtheria is the Klebs-Löffler bacillus, yet the absence of it in many cases suffering from the disease, and, on the other hand, the presence of it on many apparently healthy mucous membranes, surrounds this organism with a certain amount of uncertainty, and too often puts doubt into the minds of those whose work has not led them into a personal study of the organism. Thus a certain amount of interest is always attached to a systematic bacteriological investigation of any large number of apparently healthy throats and noses. It has been estimated by the New York City Health Department that virulent diphtheria bacilli were present in about 1 per cent. of healthy throats in New York city at the time the investigations were made;* and Dr. William Welch reports that, out of 330 healthy mucous membranes examined, he found about 2 per cent. with virulent Klebs-Löffler bacilli, and about 7 per cent. with organisms morphologically identical with this bacillus, but showing no virulence. This bacillus, furthermore, he states, "is the genuine bacillus devoid of its virulence," and not a pseudo-bacillus.

In the Children's Hospital, of Boston, during the six months, from December 1, 1895, to June 1, 1896, culture examinations were made of the throats and noses of all cases entering the hospital, two cultures, twenty-four hours apart, being taken on admission, and subsequently repeated once weekly as long as the case remained in the house, unless the

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*Scientific Bulletin No 1, Health Department, New York City.

Klebs-Löffler bacillus was found, in which case daily examinations were made until three successive negative cultures, twenty-four hours apart, were obtained. The work was undertaken with the object of preventing outbreaks of epidemics of diphtheria, which in past winters had occurred in this hospital with most disastrous results, and the success in accomplishing its purpose has already been referred to in an article on "Immunization of Antitoxin," by Dr. F. Gordon Morrill.*

The methods used in making the examinations were, briefly, as follows: In obtaining material from the throats and noses, sterilized cotton swabs were used, it being clearly evident that in studying cases in which there were no signs of infection, these were preferable to the platinum wire, as by them a larger surface of mucous membrane is touched, and the chances of reaching the desired organism proportionately increased. In growing the cultures the usual mixture of Löffler—one part of sugar bouillon and three of beef-blood serum—was used. The advantages offered by this medium for diagnosis are so great that in only one or two cases was it thought necessary to put the cultures through any of the other media. Having been thus grown for from sixteen to twenty-four hours on blood serum, the specimens were stained by Löffler's alkaline methylene blue, and in all cases where diphtheria was found the diagnosis was verified through staining by Hunt's method. This latter method, now largely used at least in this locality, consisting of a double stain of methylene blue and gentian orange, with tannic acid as a mordant, is particularly efficient in bringing out the dots which characterize the organism in question. For the inoculation of guinea-pigs, pure cultures were obtained on bouillon, and from a half to two cubic centimeters of these were injected hypodermically into the subcutaneous tissue of the abdominal wall. In all cases the second generation of the organism from the throat or nose was used for injection. Before placing the original serum-tube in the incubator, dilutions were made on blood-serum plates, from which on the following day colonies could be picked and grown in bouillon.

From the investigations conducted on these lines it was shown that out of 316 cases—the total number examined—26 at one time or another showed the presence of the Klebs-Löffler bacillus. This gives a percentage of 8.2. Out of these 26 cases showing the organism two may be said to have had clinical diphtheria. Omitting these two, we may say that out of 314 normal throats and noses examined 7.9 per cent. contained the bacillus of diphtheria. The average persistence of the bacillus on the mucous membrane was fifteen days; the shortest time was one day, and the longest 103 days, or over three months. Of these cases the nose was the principal habitat in 17 cases, or 65 per cent., the throat in the remaining 35 per cent. In one case, the Klebs-Löffler was found, together with other organisms, in the ear, the child suffering from otitis media, which may or may not have been due to this bacillus.

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* Boston Medical and Surgical Journal, May 21, 1896.

Of the twenty-six cases in which the organism was found one is deserving of special notice, from the length of time in which the infection persisted. Welch* has mentioned a case in which the bacillus was found for seven consecutive weeks, and Osler† a case where it was known to remain for eighty-four days. The case in the Children's Hospital was as follows: Daniel C., aged 9 years, was discovered having the organism in his nose on January 31, two negative cultures twenty-four hours apart, having been obtained one week before. From this time until May 13 daily cultures showed, with a few exceptions, to be subsequently mentioned, the persistence of the organism on the nasal membrane. On May 14, 15 and 16 no Klebs-Löffler bacilli were found, and it was thought by those in charge of the case an opportune time to rid the hospital of his presence. He was, therefore, discharged. Prior to this time, it is interesting to note, on several occasions, the bacilli seemed about to disappear. On February 25 and 26 none were found; on March 13 and 14 cultures were negative; and, again, on March 29, 30, and 31, three consecutive days, no diphtheria bacilli were found. But always on persistent search the bacillus was again demonstrated, often growing profusely in the tube. The virulence in this case was first tested by a culture taken on April 1. Two pigs were inoculated and both died within forty-eight hours, the autopsy showing the usual lesions of experimental diphtheria. Cultures taken on May 2 proved fatal to one guinea-pig in four days. Finally, a culture inoculated into two pigs, on May 12, seemed to possess little or no virulence, the only reaction being local induration, and in one pig, ulceration.

As regards the virulence of these organisms, it is unfortunate that lack of time and facilities did not permit of the isolation and inoculation of the organisms from a large majority of the cases, and it is, therefore, impossible to state what percentage of the total number were virulent. It has, however, heretofore, been fairly well demonstrated that in cases having suffered from diphtheria, virulent bacilli often persist for some weeks on the mucous membrane. In a long series of inoculation experiments, reported by J. H. Wright,‡ several cases showed marked virulence after being obtained in culture from the cases for from four to six weeks. As regards cases where there is no past history of the disease, as was the case in our series, Welch reports about 2 per cent. of virulent organisms out of 9 per cent. in which they were found to be present. The few experiments which were conducted by us in this line were done through the courtesy of Professor Ernst, in the Bacteriological Laboratory of the Harvard Medical School. Of the five cases inoculated, some virulence was present in four, the time of death varying from thirty-six hours to fourteen days after inoculation.

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* American Journal of the Medical Science Vol. CVII, p. 427.

† Archives of Pediatrics, Vol. XII, p. 528.

‡ Boston Medical and Surgical Journal, October 4, 1896.

It may be stated in conclusion that our experiments seem to fall well in with past investigations toward showing that diphtheria bacilli, virulent and non-virulent, are present in an unfortunately large percentage of apparently unaffected nasal and pharyngeal mucous membranes.—Univ. Medical Magazine.

IDEAL CATGUT STERILIZATION.

By N. SENN, M.D., Ph.D., LL.D.,
Professor of Practice of Surgery and Clinical Surgery, Rush Medical College;
Attending Surgeon Presbyterian Hospital; Surgeon-in-Chief
St. Joseph's Hospital, Chicago.

The aseptic absorbable ligature is one of the greatest achievements of modern surgery. In his first communication to the profession on this subject Lister alludes to the advantages of the aseptic ligature as follows: "If the antiseptic ligature be employed it merely inflicts a wound or injury upon the vessel, without introducing any permanent cause of irritation. The injured part, therefore, becomes repaired after the manner of a subcutaneous wound, without passing through the process of granulation and suppuration, which is induced by the employment of the ordinary septic ligature. Perhaps no other surgical procedure has ever enjoyed the confidence of the entire profession throughout the civilized world to the same extent as the aseptic ligature. The universal faith in the reliability and safety of the aseptic ligature is only a natural outgrowth of the superior results following its use. Protracted suppuration in wounds, the result of retained ligatures, secondary hæmorrhage, and suppurative inflammation of vessels, have been gradually diminishing in frequency, and bid fair under the influence of the aseptic ligature to be almost completely expunged from the future category of wound complications. Nussbaum very appropriately remarks: "Catgut is without doubt Lister's greatest discovery." And again: "How pleasant it is to cut the ligatures short and leave them unconcerned to their fate in the wound! In ovariotomies, etc., their value cannot be overestimated. The cases in which catgut adheres to an artery, forming connections with it and the surrounding tissues, assisting at the same time in forming a firm ring around the coats of the artery, are exceedingly welcome occurrences, guarding against secondary arterial hæmorrhage in ligating in the continuity of a vessel, and rendering even the application of a ligature in close proximity to a large collateral branch void of danger. All this silk cannot do." The advantages of the aseptic animal ligature are now generally recognized. Lister's crude method of rendering catgut aseptic has been variously modified during the last quarter of a century. Different methods have been devised and nearly all of the antiseptic substances have been employed in the preparation of catgut. The very fact that so many different methods of preparation have been employed is the very best and most convincing proof that none of them have proved entirely satisfactory. Kocher abandoned the use of his juniper catgut. Carbolized sublimated and chromicized catgut have been used very extensively, but every surgeon knows from actual experience that not infrequently wound infection could be traced to imperfect sterilization of the material. Dry sterilization of catgut seemed to become the general procedure a few years ago, but extensive trial has shown that it cannot be relied upon in rendering the material absolutely safe for practical use. The many failures of catgut as an aseptic suture and ligature, as heretofore prepared, are responsible for the substitution of silk for catgut in the practice of many surgeons. Silk can be readily sterilized by boiling, the simplest and quickest method of effecting absolute sterilization.

The ideal sterilization of catgut consists in rendering the material not only absolutely sterile but also mildly antiseptic without impairing its tensile strength. Every surgeon has been anxiously looking for a method by which catgut could be prepared so that it could be sterilized by boiling without impairing its strength. Fortunately this hope has at last been realized. Experiments have shown that catgut and leather immersed for forty-eight hours in a 2 to 4 per cent. solution of formalin undergo an unknown chemic change which alters their texture in such a way that its tensile strength is not impaired but rather increased by boiling. The commercial catgut is subjected to the action of the formalin without any previous preparatory treatment of the raw material.

Hofmeister, who has done such excellent service in perfecting the formalin preparation of catgut, gives the following most recent method: 1. The catgut is wound on a glass plate with slightly projecting edges, so that the gut is free from the sides of the plate and exposed to the circulation of the boiling and flowing water. The ends of the gut are fastened through holes in the plate. 2. Immersion twelve to forty-eight hours in aqueous solution of formalin 2 to 4 per cent. 3. Immersion in flowing water at least twelve hours to free the gut from the formalin. 4. Boiling in water from ten to thirty minutes. Ten to twelve minutes is amply sufficient, as all microbes and spores are killed by exposure to boiling heat for that length of time. 5. Hardening and preservation in absolute alcohol containing 5 per cent. of glycerine and one-tenth of one per cent. of corrosive sublimate.

Experiments on animals with catgut thus prepared have proved that it is absorbable, though not as quickly as the ordinary material. One of the essential conditions of success in this method of catgut sterilization is to wind the gut quite tightly around the glass plate or hollow glass cylinder during the process of sterilization.

The first attempts to sterilize catgut by this method under my own direction were made at the St. Joseph's Hospital by the sister in charge of the operating room. The result of experience has led us to modify the procedure in several ways. Instead of glass plates ordinary abdominal glass drainage tubes have been employed, upon which the gut is wound quite tightly. These glass drains have been found an excellent substitute for the plates. An ordinary large test tube would answer the same purpose. The remaining directions given by Hofmeister were followed to the letter. Numerous inoculations with fragments of catgut prepared by this method in sterile gelatine invariably gave negative results. The catgut is as strong as the raw material, hard, and the knot is less liable to slip than when the ordinary material is used. We have also ascertained that the formalin catgut can be reboiled almost any number of times without impairing its strength.

Catgut to be safe should not only be absolutely sterile but should contain a sufficient quantity of efficient antiseptic to render it unfit as a culture medium for pathogenic microbes. Hofmeister renders it antiseptic by immersing it in an alcoholic solution of corrosive sublimate. Others have substituted carbolic acid for sublimate. Both of these antiseptics unduly irritate the tissues and increase the primary wound secretion, effects which cannot fail in interfering to a certain extent with an ideal healing of a wound by primary intention. The valuable and interesting experiments made recently by Lauenstein leave no doubt that it is almost next to impossible to render the field of operation absolutely aseptic by any of our present methods of disinfection. We are forced to admit that nearly every wound inflicted by the surgeon's knife contains some pathogenic microbes notwithstanding that the strictest aseptic precautions may have been carried out. The experiments made by Ewald have also furnished positive proof that sterile catgut often contains a sufficient quantity of an unknown toxic substance which by its destructive action upon the cells engaged in the reparative process transforms them into pus corpuscles, resulting in the production of a limited aseptic suppuration and the formation of sterile pus. Undoubtedly many of the stitch abscesses which occur in the practice of painstaking aseptic surgeons have such an origin. These experimental researches force upon us the conclusion that catgut should not only be sterilized but that it must be made sufficiently antiseptic to at least inhibit the growth if not destroy the pyogenic microbes which enter the wound during the operation or which may reach it later through the circulation. In this part of the preparation of catgut I have modified Hofmeister's method by substituting for the corrosive sublimate iodoform. After boiling the deformalinized catgut for twelve to fifteen minutes it is cut into pieces of desirable length, tied into small bundles containing from six to twelve threads, when it is immersed and kept ready for use in the following mixture: Absolute alcohol 950, glycerine 50, iodoform (finely pulverized) 100. The alcohol dissolves part of the iodoform. The bottle containing the catgut should be closed with a well-fitting glass cork and should be shaken well every few days to bring the dissolved iodoform in contact with the threads. The catgut can be kept in this mixture for any length of time without losing its strength. One of the valuable properties of iodoform applied to a recent wound is to diminish the amount of primary wound secretion. It does not destroy pus microbes, but inhibits their growth. I have used catgut prepared by these modifications of Hofmeister's method with the most satisfactory results, and shall continue to use it until some better method is devised.

In conclusion I would advise hospital authorities and surgeons to prepare their own catgut by this or similar processes and not rely upon manufacturers for their supply.

PROCEEDINGS OF SOCIETIES.

THE NEW YORK MEDICO-SURGICAL SOCIETY.

January 4, 1897.—Diastase in Therapeutics.

(Vide 1st Original Article this issue.)

DISCUSSION.

Dr. W. L. Stowell: I wish to express my pleasure at listening to this paper. Medical societies have spent so much time of late in discussing contagious diseases and brilliant operations, that the more common diseases have been largely overlooked. We should, therefore, thank the reader of the paper for bringing up this subject. Difficulty of digestion is not epidemic—it is pandemic; it is with us all the time, for people eat more than they should, as a rule.

For the last five or six years I have been greatly interested in the various forms of indigestion and the remedies supposed to overcome these. Most of the diseases we are called upon to treat, if not due to direct infection, are due to some difficulty in the digestive tract. These difficulties are either fermentation from pathogenic organisms or deficiency of digestive ferments. Antiseptics, while they prevent the formation and development of germs, and so prevent indigestion to some extent, do not assist the digestion of food in a weak digestive tract. Therefore, we need to help the patient by first selecting suitable food, and secondly, by helping him to digest what food is taken in.

For a long time I have used the various malt extracts for their diastatic properties, some claiming them to have great diastatic power, and others not. As a matter of fact, the malt extracts go far towards increasing nutrition, if not nutritious in themselves. Unfortunately if large quantities of malt are given, indigestion arises from this cause. I find many patients rebel against malt; adults often objecting to the taste of it, to say nothing of the effect upon the stomach.

Another useful remedy for starch indigestion is Taka-diastase, as it is claimed to have the power of digesting over a thousand times its weight. Certainly a dose of two or three grains will very materially aid in converting the starch, and so minimizing the flatulence and indigestion, and the accompanying disagreeable symptoms.

For some time I did not obtain as good results as I have more recently, probably because I had not learned to distinguish between starch and proteid indigestion. Patients who complain of bloating and pressure an hour or two after meals, and who complain also of constipation most of the time, can, as a rule, be greatly helped by diastase alone. If this does not work well alone, it will do better when combined with salol, or some similar antiseptic. The more I have used these remedies the better I have been pleased with them.

I have been interested in the great variety of symptoms exhibited by different individuals and different constitutions. It requires much close questioning in the case of a child to determine what article of food is chiefly responsible for the indigestion. Diastase is just as good for the nervous individual with indigestion as for a person of phlegmatic temperament, with sluggish secretions and indigestion.

I recall an interesting case of vomiting during pregnancy. The patient suffered from inability to digest both proteids and starches, but particularly the latter. She suffered very greatly from the resulting distention, but this was overcome by the administration of Taka-diastase. After awhile the stomach rebelled, but then by washing the stomach it became possible to return to proteids. One or two patients have come to me from a distance suffering from an aggravated form of starch indigestion, and these have done exceedingly well upon diastase. One of them improved with diastase alone; the other did better when it was combined with salol. Our trouble with the digestive ferment is that we do not pay sufficient attention to the different forms of indigestion, usually preferring to depend chiefly upon a routine dose of rhubarb and soda.

Dr. William A. Walker: I desire to express my gratification at being present to-night and listening to this very interesting paper. We certainly owe Dr. Fite a good deal for having brought forward so clearly the scientific side of this subject. Most of us are not in a position to study the scientific side. Laboratory work should go hand in hand with our clinical work, but most physicians must depend upon the regular laboratory workers for such knowledge. It is the physician's province to put this knowledge into practice at the bedside.

In general practice we certainly meet with a large number of cases of dyspepsia. Americans have the reputation of being a dyspeptic race, and this is certainly true. It is not unusual for a business man to come to us complaining of an attack of indigestion, coming on—not in the morning, as a rule—but in the afternoon after the midday meal. They usually complain of heaviness, stupor and languor, and inability to concentrate attention on the work before them. In many of these cases the trouble is due to weariness after the morning's work, the taking of a hurried luncheon and the hasty return to work. Generally the articles of food for the midday meal are not well selected, but we cannot get them to make a proper selection. What are we to do for these people who complain of distention and bloating an hour or two after the meal? I have had the most satisfaction from the use of Taka-diastase. One advantage is that it is tasteless. I prefer to give it in capsule, because if administered in the dry form, the patient usually complains of a dryness of the throat. The malt extracts have the disadvantage, as has been said by Dr. Stowell, that the patient very soon tires of them; they become disgusted with the sweet taste, and the bulkiness of the dose. This holds good, no matter in what vehicle the physician may direct that the malt extract be administered.

The question is often asked: What is the advantage of giving the diastase? It has been argued, principally on theoretical grounds, that the giving of an artificial digestive agent cannot produce a cure, but that it simply does a certain amount of work which the stomach should perform. I think Dr. Fothergill spoke of this practice as "pauperizing the stomach." This may be true in long-standing cases, but it certainly does not apply to the most acute cases. Most of its good effect is produced by supplementing the digestive ferments of the stomach. I have found in certain cases that after washing out the stomach there would be left over from the previous day portions of the food. I have then given the diastase, and have found, on subsequent washings, that a good portion of the food had been digested. The cure is effected, I suppose in this way. Most of the trouble in dyspeptics is probably due to the leaving over of some undigested food from one meal to another. The diastase prevents this.

Again, it is often very difficult to know how to feed children who have been just removed from the breast or the bottle. Mothers are not usually very particular about what the child takes at this period. Here we often find a furred tongue, a bloated stomach, constipation, and an unhealthy child. If we can induce the mother to restrict the diet, and give some form of diastase, we are likely to restore the child to health. I have found it a good plan to soften zweiback with water—not milk—and then sprinkle a little Taka-diastase over it. Or, a little diastase can be stirred up with almost any of the ordinary morning foods, and given to the child without its knowledge.

Dr. George Tucker Harrison: This is a subject in which I have taken a great interest for some time. Like most clinical observers, I often meet with these cases of indigestion, which in former times, we erroneously looked upon as due to proteid instead of amylaceous indigestion. I, therefore, hail with pleasure, as a great addition to our therapeutical resources, this new diastase—a substance which certainly appears to be worthy of extended and careful trial. The surgeon has of late been giving more attention to this subject, because he has learned that something more is necessary than to perform a brilliant laparotomy for appendicitis, or extrauterine gestation, or what not. Doubtless patients have been lost from ignorance of the physiology of digestion.

Only two or three years ago, I saw in consultation with a friend, who had performed a Cæsarian section, a case in point. The patient was very nearly lost because of an enormous distention of the abdomen, due to the development of gas in the intestine. Fortunately, the distention was relieved by lavage of the stomach. But in many of these cases we must pay more attention to the physiology of digestion, in order to be able to choose a proper diet for them.

Since Dr. Fite called my attention to this new diastase, I have used it considerably, and have been much gratified with the results of my observations. In one case which comes to my mind—a lady of fifty-five years had taxed my therapeutic resources to the utmost. I had used, among other things, a preparation of nux vomica, pancreatin, and fel bovis inspissatum. This utterly failed. I began then the use of Taka-diastase, and the result was truly brilliant. The woman suffered from the usual symptoms of intestinal indigestion, flatulence, colicky pains after eating, mental depression, insomnia, etc. In other cases, I have met with equally good success.

I was particularly interested in Dr. Walker's remarks on the use of diastase in infant feeding. These cases have perplexed me beyond measure. Probably there is not a practitioner present this evening who cannot recall a number of cases in which he has felt that the child must starve to death because he was unable to feed it.

My friend Dr. Wilcox says that the trouble with these liquid malt extracts is that, owing to the quantity of alcohol they contain, and the development of acids consequent upon fermentation, the action of the diastase is inhibited.

Last summer I had a child who had to be artificially fed. I made use of the Walker-Gordon method of feeding with no success. I used sterilized milk and barley water with no benefit whatever, and a similar result followed the trial of all the usual preparations. The child was emaciated to skin and bones. I then resorted to milk with a superior quality of malt extract. I have never seen a child thrive as this one did. I believe it was entirely due to the action of what diastase was contained in the malt extract.

Dr. Henry Dwight Chapin: Unfortunately I was unable to get here in time to hear Dr. Fite's paper, but I am familiar with his views on this subject, particularly as regards infant feeding. The remarks of Dr. Harrison have interested me especially, for his clinical experience makes these observations carry much weight, although theoretically I cannot understand how the results were obtained.

There are two well-known methods of acting on the casein of cow's milk for infants, viz.: (1) the mechanical; and (2) the physiological. The mechanical method consists in adding some diluent, such as barley water, or the old "flour ball." I think that, on the whole, we get better results by mechanical means than by peptonizing the milk. When the method of peptonizing milk was first brought out, most of us thought that the problem of infant feeding had been largely solved. I tried the method with much enthusiasm, yet I found that the simple peptonizing of milk, did not, on the whole, give very good results. In some difficult cases it certainly does succeed, but this has not been the rule in my experience.

This has led me to go back to the old method of attenuating the milk by the addition of cereals. We all know that young infants cannot digest much starch, although they can digest some by means of the mouth secretions and the secretion of the intestinal mucous membrane. I made a series of experiments at the laboratory, and at the bedside regarding the action of diastase on barley. I think we succeeded in this way in getting the good effect of the attenuating qualities of the barley, and at the same time, in reducing to a minimum the bad effects of the starch. Clinically, this method gives good results in very many cases. The process is not a difficult one, and this in itself, is a recommendation for the method, for in infant feeding it is highly important that the rules for the preparation of the food shall not be complex. Even skilled nurses often make mistakes in the process of peptonizing milk. At times, there may be trouble from a weakness in the ferment itself.

In many cases, by diluting fresh cow's milk with barley or wheat flour, after the starch has been treated with diastase, we shall get about as satisfactory results as from any other method of artificial feeding.

Dr. Morris Manges: My own experience strongly corroborates that of Dr. Walker as to the value of diastase in infant feeding. How it acts, I do not know, but I know that it does act. With children who do not thrive on breast milk, or substitutes for it, I can say that the addition of ten drops of maltine, or more recently, the use of Taka-diastase, has given me uniformly good results. This is a simple clinical fact.

I believe the use of preparations of diastase has been of service in one way particularly, *i. e.*, in calling our attention to the importance of the saliva in digestion. We had almost lost sight of its value, notwithstanding the fact that it is secreted in such large quantities. We must admit that so far as experimental work has added to our knowledge we are still much in the dark regarding this important fluid. Just to what substance in the saliva the action on the starch is due, aside from the ptyalin, we do not know. Certainly it is not the ptyalin alone. The experiments of Jawein, of Vienna, and of others, have shown that ptyalin was lessened in certain acute infectious diseases, diabetes, renal and cardiac diseases; but strange to say, it was not at all lessened in tuberculosis. On the other hand, experiments by Friedenwald have shown that saliva can be boiled and the ptyalin thus killed, and yet the saliva will have a beneficial action in promoting starch digestion. It is evident, therefore, that other constituents besides ptyalin are at work.

Now, by giving Taka-diastase, can we really replace the saliva? That a ferment is, after all, not the original digestive fluid is a point which is very often lost sight of. Every single digestive juice shows us that there is something more than the ferment which is active. Just what important role is played by the carbonates, and by the nascent oxygen in the mouth is not yet known. This recalls to my mind an experience with a chemist in Berlin, who was working with very delicate test papers for nascent oxygen. He found that if he used large quantities of saliva, by means of chewing gum, he obtained a most delicate reagent for nascent oxygen.

However, there are other factors to be considered. In the mouth there is a condition of fermentation going on, often leading to acidity. We must be sure of the condition of the mouth before resorting to the administration of ferments. To one who has not investigated this subject, the condition of the mouth is surprising. Dentists understand this matter better than physicians. Another important factor is the selection of the food. If we can accept the term, "amylaceous dyspepsia," it is rational, (1) to take care of the mouth; (2) to select proper food; (3) to insure the secretion of a large quantity of saliva by Nature's means—through mastication of the food; and (4) to resort to ferments. I would not resort to ferments before that, because the pernicious habit of "bolting" the food would then be rather encouraged. I think, after all, Fothergill was right in saying that these remedies are crutches—although certainly very valuable ones—and hence, that they should be used only as temporary agents. It would be well for all of us to repeat the old experiment of adding saliva to starch, and then comparing this with the effect of adding diastase. I think we should hold the diastase preparations in reserve until other methods have failed. Alkalies ought to be associated with diastase, especially in cases of hyperacidity, in order to neutralize some of the excessive hydrochloric acid in the stomach, to permit the continuation of the action of the ferment on the gastric contents.

Dr. William Henry Porter: I do not quite know why I consented to come here to-night and take part in this discussion, for I am afraid I am not in a position to discuss Taka-diastase—which I have no doubt is the best preparation of diastase yet produced—or even diastase as a therapeutic agent. The subject of indigestion and of intestinal indigestion in particular, is one of the most interesting ones in medicine, to my mind, but I must confess that I have had very little experience with any of the diastasic ferments. If I wish to correct starch indigestion, I get rid of the starch first of all. In the adult it is a comparatively easy matter to get rid of the starch. In my own practice, I rely very largely upon the proper regulation of the diet—a proper regulation of the food as regards both quantity and quality. I think we must admit that the animal organism is capable of digesting a certain quantity of material if up to the physiological par, and that if below this point, it is not able to digest such a quantity. In such a case, we must cut down the quantity to be digested. If we do this, we shall find that the indigestion will disappear. I believe that the statement that Fothergill makes, that it is wrong to use artificial ferments, is a wrong view; for, in my opinion, when an individual has fallen below physiological par, in the ability to produce ferments there is also a deficiency, and that they never can digest enough food to keep the system up to par, and so repair the damage. By using these ferments we can aid the stomach so that a proper supply of nutritive pabulum is taken into the blood. By doing this we increase the ability of the digestive organs to manufacture ferments of their own. We can then materially cut down the artificial ferments used, and the individual will quickly return to the normal condition.

There is one idea which is very prevalent in connection with starch, and which I think is a very erroneous one and that is, that starch is a fat-producing element *per se*. I do not believe the animal organism ever converted starch into fat. If what the chemists say is true, that statement is a correct one. They say that when you commence to split up the starch compounds in the animal organism, they tend to become less complex, and that synthesis does not occur in the animal organism. Therefore, I believe we must give up the idea that fat was ever made out of starch. The only reason for that belief was an experiment made many years ago, in which two pigs were taken from the litter, both pigs being of about the same weight. Pig No. 1 was cut up and a quantitative estimate made of the fat. Pig No. 2 was fed on starch, and nothing else for a week or ten days. It was then killed, and a quantitative analysis showed a little more fat than in Pig No. 1. Consequently, the statement has been handed down for years that starch is converted into fat. I believe that all the fat we get in the animal economy comes from the katabolic destruction of proteid bodies, and I think we have pretty good clinical evidence of that. Where we cut off every bit of starch and use simply proteids, sugar, and fat, these people, in the absence of starch, will progressively increase in weight. This comes, I believe, from a better utilization of the proteid elements, and not of the starch and fats. I believe that the fats and the starches are simply heat producing elements. A proper adjustment of the starches and fats is necessary, and when we cut off the starches I believe we must substitute a little alcohol to generate the heat in about th same proportion as would be generated if we used the starch. A certain number of grammes of starch are capable, for instance, of producing a certain amount of heat. If we deprive the system of that amount of starch, we must get that equivalent of heat out of alcohol.

If we consider many of these numerous little points I think we can very satisfactorily manage these cases of indigestion. Personally, I do not like the terms "albuminous," or "amylaceous," or "fatty" indigestion. I think the difficulty is either a gastric one or an intestinal one, and therefore, in my opinion, these are the only two kinds of indigestion that we should consider from a clinical standpoint. The sooner we come to this simple method of considering the subject, the quicker we shall arrive at a more rational basis of treatment.

Dr. Charles Henry Brown: The subject of ferments is a very large one, yet very little has been left after the reading of this paper for the practical physician to discuss. Those who have used Taka-diastase are aware that it is a diastase, and that it will artificially digest starch in the test-tube, and also in the fermentative forms of dyspepsia. The subject of ferments begins with the dawn and ends with the night; we see it at birth, and we see it at death; we see it bringing health, comfort, distress, disease. It is a subject which we physicians must understand and apply intelligently. We have heard to-night regarding the use of diastase in the feeding of infants. Practically, it aids in the digestion of amylaceous substances. I have used for years some form of diastase in the artificial feeding of infants, from birth until they are able to take food independently of the bottle, and I think it is a very great aid to us in the frequent but puzzling idiosyncrasies of infantile indigestion. The question Dr. Porter has raised as to whether starch alone makes fat or flesh is not the one for discussion—it is rather the chemistry of the action of diastase, which undoubtedly produces a certain amount of flesh. If we give pure alcohol, we do not see it; if we give malt, we do see it. People at the time of life when the heat-producing mechanism is most active, put on flesh. A certain force produces this flesh and a storing up of fats.

In Taka-diastase I think we have a better preparation for treating certain forms of indigestion in adults than any other. It is a powder, and the diastase is measurable. The addition of alcohol interferes seriously with the digestive action of diastase. All of us know that malts which contain a large quantity of alcohol often increase the distress of dyspeptics. There can be no question that Taka-diastase is a remedy which we can use either internally, or in pre-digesting food. We can also use it in aiding nutrition. Undoubtedly, the use of diastase increases nutrition, probably by stimulating the proteids and increasing heat-production by its chemical action on starch. I have found that delicate infants, when given Taka-diastase in proper quantities, will show the result of this stimulant action, as well as delicate adults. When given symptomatically, it relieves the distress experienced in the various forms of amylaceous dyspepsia. This seems to me to cover the whole subject, so far as it is at present known.

Dr. Glover C. Arnold: What are the maximum and minimum doses of Taka-diastase for adults and infants?

Dr. Fite: In experiments at the laboratory it seemed that one grain converted fifteen hundred grains of starch in three hours, one hundred grains in ten minutes. After extended observation it was determined that two grains and a half, given during a meal, was an average dose for an adult. Some physicians have used five grains at a dose, but this has appeared to cause too rapid a conversion of the starch. It seems that the doses for infants are proportionately larger, *i. e.*, one grain for a child of two years.

Dr. William H. Porter: Were these laboratory experiments made with starch and diastase alone, or in conjunction with proteid matter, etc?

Dr. Fite: With all the digestive ferments from all sources and on starches and proteids. Dr. Porter spoke with reference to making fat from proteids. I would like to take him on a farm and let him see how long it would take him to fatten a hog with meat alone.

Dr. Porter: I would like to go with Dr. Fite on a farm and let him try to fatten a hog on starch and sugar, and take out the proteid matter. We must remember that there is proteid matter in corn and grain.

Dr. Harrison: The experiment has been tried on a very large scale, and I think it has been proven in an exact, scientific manner that sugar does produce fat. Experiments have been tried on the sugar plantations in Louisiana, and every man who knows anything about it, knows that the "little niggers" all get fat when they are boiling sugar, and get plenty of molasses and sugar cane.

Dr. Porter: There is no question about the fact as stated, but it does not alter the situation of the chemist. An individual is capable of taking in normally 750 grammes of oxygen daily. Starch, sugar and fat burn more rapidly than proteids, and if they are taken in excess, you exhaust the oxygen supply; there is insufficient oxygen to oxidize the proteids. As a result, one of the katabolic products of the imperfectly oxidized proteids is fat, which of necessity accumulates as fat in the system.

Dr. John Blake White: I should like to speak with reference to the practical aspect of the subject. Theory is beautiful indeed, and is most fascinating, but actual results, and the practical side are, after all, more important. I cannot discuss chemistry with Dr. Porter, but I can speak of the results obtained in practice. These substances certainly do produce fat. Whether this can occur from the chemist's standpoint, or not, I cannot say, but the fact remains.

I desire to add my testimony to the value of assisting faulty digestion by these means that have been discussed here this evening. My experience with Taka-diastase, and other digestive ferments, tends to confirm the position taken by the essayist. I have always looked upon my cases from a purely practical point of view, endeavoring to ascertain what was really at fault, and then remedying this. I endeavor to regulate the diet. In cases where the indigestion appears to be due to the defective digestion of starchy matter, we can resort with advantage to the use of preparations of diastase. The weight of authority certainly favors such a view.

Dr. Fite closed the discussion. He said: I wish to thank you, gentlemen, for the interest you have shown in the subject presented in my paper. Dr. Manges alluded to the good work of Dr. Friedenwald of Baltimore with saliva. I have had the pleasure of discussing this matter with Dr. Friedenwald who is a high authority in this line of work. So is Dr. Manges.

RICHMOND ACADEMY OF MEDICINE AND SURGERY.

Regular Meeting, December 8, 1896.

Dr. Landon B. Edwards, President, in the chair. Dr. Mark W. Peyser, Secretary and Reporter.

Dr. Stuart McGuire read a paper on "Residual Urine of the Urethra." (See page 138.)

DISCUSSION.

Dr. J. W. Henson said, that there was such a thing as residual urine of the urethra, there could be no doubt, and it was a matter of considerable importance. While residual urine caused posterior urethritis, he was convinced that local posterior urethritis might and did bring about the condition of residual urine in the urethra.

The surgical posterior urethra was emptied by the rhythmical action of the levator prostatæ, compressor urethræ, accelerator urinæ, and the muscular fibers surrounding the urethra beneath the submucous coat. When there is a point of inflammation, there is, of course, some swelling; but, added to this, the muscle at that point, or some of its fibers, assumes a spasmodic action, the rhythm above mentioned, is interrupted, and the deep urethra fails to be completely emptied. Question an intelligent man, and he will tell you that after passing urine that is highly concentrated, he is sometimes conscious of being unable to empty the deep urethra for ten minutes or more, when, the local irritation having subsided, the same muscular effort at first used easily accomplishes the act. Now, of course, when the local irritation is continuous, as in inflammation, the spasm is longer or continuous, with retention of urine back of the irritated point. Residual urine is oftenest only a few drops, but this, by decomposition, is sufficient to greatly intensify the inflammatory action and hasten the formation of stricture. He thought it not improbable that urine was retained by the sharp bending of the urethra which occurs just in front of the scrotum when the penis is pendulous. This would occur, of course, only when the organ is replaced before being thoroughly emptied after urination.

In order to properly grasp the situation of affairs in posterior urethritis, or any urethritis of a chronic character, and intelligently treat the same, the use of the endoscope is necessary. To attempt to manage a case without this instrument, would be a much more serious blunder than treating a sore throat without inspection. Many a patient has been told he had nothing the matter with his sexual organs, while the fact was, he had, and, as a result, nearly or quite became a lunatic. The reverse is equally true. It is a serious matter. He congratulated Dr. McGuire upon his paper.

Dr. W. T. Oppenhimer agreed throughout with Dr. McGuire regarding the employment of the endoscope. The urethra was a closed tract, not admitting of air, and the folds might be seen closing behind the instrument. Minute inflammatory points as results of gonorrhœa, uric acid crystals, etc., residual pus, mucus that might be mistaken for urine, all could be found. The urethra should be fully dilated with the instrument, so that ulcerations might not be hidden by the folds. The endoscope was certainly a great advance in the treatment of urethral troubles; but it must not be introduced in acute inflammations. In his experience, deep injection of a solution of atropine. stopped secretions, and in the more acute forms, he used it in combination with other remedies.

Dr. Stuart McGuire, in closing the discussion, said that the paper he had read had been hurriedly written, and that it was merely intended to be suggestive. His object in reading it was to endeavor to establish a clinical fact, namely, that in certain cases of chronic posterior urethritis there was a retention of a considerable quantity of urine in the urethra, which was either the cause of the trouble, or a complication which made it difficult to cure. He dwelt upon the importance of using the urethroscope in such cases, and exhibited various electrical illuminating apparatus, and demonstrated, practically, their operation. He concluded by urging the profession to be more accurate in their work, and begged that in future they would not diagnose urethral symptoms as neuroses until by a careful examination of the entire length of the urethra they had demonstrated the fact that it was free from abnormalities.

REPORTS OF CASES.

Dr. Jacob Michaux spoke of a case treated for diphtheria, although the diagnosis was obscure, and it was some time before he was satisfied as to its nature; but he quarantined it from the beginning. The membrane on the left tonsil disappeared in a day or two, and as there were no symptoms of depression, the patient was allowed to get up. In a week, the membrane on the right tonsil began to disappear. Monsel's solution, undiluted, was applied, marked and decided improvement resulting. The case ended in recovery. He did not claim originality for the treatment.

Dr. Arthur Jordan described a case of tinea versicolor, which was said by a former attendant to be syphilis. If there be any doubt, an examination of the scales (which could be scraped off with the finger nail) under the microscope, would show the mycelium with spores. The direct cause is purely parasitic, and is the microsporum furfur which grows in the epithelium and the characteristic feature is the network of mycelia in which may be seen the spores. In treating the disease, he advised thorough boiling of the clothing; the application of green soap and the local use of:—

℞—Hydrargyri chloridi corrosivi	gr. vj.
Alcohol	ʒ v.
Hydrargyri ammoniatis	gr. x.
Aquæ rosæ	℥ vj.—M.

In three days the patient was well.

Dr. Oppenhimer said that it was of importance to the general practitioner to recognize skin diseases. This particular one, sometimes known as "liver spot," is quite common. If the most minute point were left, it would be the nucleus for a new growth. The spots become red after a bath, and iodine, which is good treatment, colors them brown.

In answer to a question, Dr. Jordan said these spots do not often appear on the face. Other diseases were xeroderma, and chloasma or leucoderma, which by the laity might also be called liver spots.

Dr. Henson reported a case with one peculiar feature. The patient had obscure pains about the chest, and sometimes in the legs. He could make out nothing but dyspepsia. At times, there would suddenly occur nausea, and when the patient was about to vomit, he would sneeze, and be immediately relieved.

EFFECTS OF PAQUIN'S ANTI-TUBERCULAR SERUM.

Dr. Paulus A. Irving said about November 1 he began treating Mrs. C., of Richmond, with Paquin's anti-tubercular serum, for incipient phthisis. It was a typical case, with the elevated temperature, peculiar indicative expression, flushed cheeks, brilliant eyes, night sweats, etc., but no expectoration—just the case, he thought, where, if there were any virtue in this remedy, one would expect to realize it. He began this treatment about the first of the month, beginning with five minims of the serum, and gradually increased it to twenty-five minims, giving this on November 18. On November 20 he called to see her, and after washing her back, getting it thoroughly clean (using 1 to 2,000 bichloride solution), injected into the connective tissue opposite the dorsal region, between the scapula and the spinal column, only fifteen minims of the anti-tubercle serum, having injected twenty-five minims two days before, getting it out of the same bottle and using the same syringe, and taking every precaution as to cleanliness, as he had done before. This was the twelfth injection of this serum since November 1. The patient, as usual, was lying on a lounge. In less than half a minute she said she felt very dizzy and peculiar about her head. He told her to put her head lower, which she did. He turned and looked at her, and noticed that she had a very peculiar flush on her cheeks and face, which quickly deepened in color until she was of a perfect scarlet hue, which rapidly spread all over her body. She soon became very nervous and excited, and complained of being chilly. She was put in bed as quickly as possible, given aromatic spirits of ammonia and whisky; mustard applied to the bowels and stomach, and hot applications to the feet. Her respiration was very hurried and shallow. She had one of the severest nervous chills he ever witnessed, being unable to hold herself still. Muscular and tendinous twitching were very severe. By this time no pulse could be detected in either the wrist or the temporal region. Her face, hands and body were very hot to the touch, her feet cold. She complained of being very cold, asking that more cover be put on her. Her pupils were dilated to the fullest extent; face very much swelled, especially beneath the eyes; upper lip greatly swelled, thick and stiff; the eyelids swelled and partly closed, and the conjunctiva injected and reddened.

The hyperæmia or scarlet hue, in about an hour, changed to a decided purplish one. She then passed into a decided cold stage and an alarming state of collapse. By the active and free use of whisky and ammonia, morphia sulph. $\frac{1}{8}$ gr., and nitro-glycerine, 1-100 grain, and mustard plasters, he succeeded in about three hours, in producing enough reaction to justify leaving her. He saw her again in the evening and the next day. She had not entirely rallied, had spent a rather sleepless night, her face was still injected and swelled, and she was confined to her bed much weakened and debilitated. It was three or four days before she was strong enough to leave bed. His object in reporting the case was not to throw any discredit on the serum treatment, but that others knowing the distressing symptoms sometimes produced by this serum might profit by his most trying experience, and to ask for an explanation of this condition. What produced this chain of symptoms? The serum was the same he had been using for the last month, and it was kept tightly sealed in a dark, cool room. The specimen was prepared in September.

The lesson that he drew from this case was that, as we (general practitioners) have no facilities for testing the purity of these articles when first purchased, or after they have been opened and used for awhile, we had better go slowly in accepting and using them. He thinks the profession should decline to use these secret remedies, for in employing them, it endorsed them.

Dr. Virginius W. Harrison reported a case of tuberculosis in which he had used Paquin's serum thirty-five times, commencing with fifteen and gradually increasing to forty-five minims. At the time of the twenty-eighth or thirtieth injection, the kidneys becoming troublesome, the amount was reduced to fifteen minims, and again gradually increased, this time to twenty-five minims, when the bladder and kidneys again became irritated. The desire to urinate was constant. At the injection previous to the last, the patient complained of tingling in one of his arms, and on the day following, and continuing for three days, he had no use of it. The serum was discontinued; but ten days before this, the whole surface of the body broke out in whelps. He was particular in washing out the syringe before using the injection. Dr. Harrison said he failed to see any benefit arising from the serum. Simultaneously with its employment, hypophosphites and creosote were administered.

Dr. Wm. S. Gordon began by remarking that Dr. Paquin had stated that no bad effects had ever followed the use of his serum. The case he was about to report had had pulmonary hemorrhages. He began with the injection of five minims, increasing the dose one drop daily. The patient became no better, but on the other hand, the slight hectic he had, developed after twenty minims had been reached, into a high fever, and there was general malaise. So the treatment was discontinued, and the patient improved. It was then resumed, the patient again declining; fever rising, etc. Discontinuing the use of the serum, he was put on regular treatment, and since that time his condition has improved. How far the bad effects were due to a deteriorated serum, Dr. Gordon doesn't know, but if it were of bad quality, there would have been cellulitis at the site of injection. In closing, he made the point that the method of manufacture of the agent had never been completely disclosed.

The President, after calling Vice-President Dr. Oppenhimer, to the chair, remarked that on several occasions, in three of his own patients, he had observed effects that might be spoken of as apnœac attacks—very much like those spoken of by Drs. Irving and Harrison. In a minute or so after injections, the patients complain of peculiar dizzy sensations, suffocative breathing, the face becomes intensely red, and this gradually passes off into a reddish purplish hue of the upper part of the bodies, with tingling sensations creeping over the body, reaching even to the toes and fingers, with numbness extending from the head down until it reaches an ascending feeling of a like kind coming from the extremities. During this period, the patients become anxious, developing a trembling of the body generally; and then the radial pulse, scarcely affected until now, becomes rapid, small, and approaching a thread-like feeling. Gradually, the symptoms pass off, and, as a rule, all unpleasant effects are gone in fifteen to twenty minutes. On one or two occasions, however, one of the patients had moderate suffocative asphyxial attacks of a few minutes' duration, occurring within the first hour or two after the injections. In none of the cases reported, however alarming the symptoms appeared to be, did any fatality occur.

The usual care had been taken in each case to keep the bottles of serum in equally cool, dark places, and the serum was from bottles freshly received from the manufacturer's laboratory. Aseptic and antiseptic precautions were carefully followed out in administration in each case.

Here, then, we have five cases of very unpleasant immediate effects of the administration of antitubercle serum which have led Drs. Irving and Harrison to give up its use in each of their cases; but have not deterred the speaker from continuing its use in his cases—because it has appeared to him that "everlastingly sticking at it brings success" in the relief of conditions that will surely prove fatal unless something can be done to check the growths of tubercles. Serum will rarely fail; tuberculosis unchecked by serum surely will. In the interests of his patients, as well as of therapeutic science, he will ever take the lesser risk.

These cases led Dr. Edwards to ask Dr. Paquin for explanation, and from him, under date of November 28, 1896, he received a very full letter from which the following is condensed:

In the directions that accompany every bottle, this very occurrence is mentioned as a possibility. In experiments with every kind of serum brought out for the treatment of specific maladies, as diphtheria, tuberculosis, tetanus, erysipelas, streptococcus infection, etc., each of them may produce sudden flushing of the face, dyspnœa, some nervous effects, etc. The experience of other scientists has been the same; but none of them can explain the reason. It seems, however, that horse blood-serum is more liable to produce these peculiar effects than the serum of a dog or some other animal when introduced into man's system. For a long time his laboratory workers have been trying to disassociate the various elements of the serum to find which one, or what combination of them, produces the effects spoken of. One conclusion arrived at is, that they are never due to septic material produced by bacteriological contamination or changes in the serum; for they have been produced by serum perfectly fresh—just drawn from the animal—filtered through a Pasteur-Chamberland bougie. If due at all to any special element that exists in the serum at any time, it must be some substance in the economy of the animal from which the blood is drawn, the nature of which we do not yet understand.

None of these accidents, however (in a record of over 80,000 injections of serum with, perhaps, 50 reports of the kind referred to—Dr. Paquin himself having noticed them several times), have resulted seriously; they have all passed away in a short time. One of his patients, who receives about 60 minims every other day, gets these same results every week. A salt solution, when given hypodermically, will occasionally produce the same thing; so that he rather inclines to the opinion that it is the amount or quality of salt in the serum that causes the trouble.

However, this whole trouble can be avoided absolutely. When a patient is so susceptible to the use of the serum by hypodermic medication, then it should be administered by rectal injections—giving sometimes the maximum dose of 60 minims, diluted in about the same quantity of tepid water or natural salt solution, making the injections in the evening on retiring. Use a simple enema to wash out the bowel before the injection of the serum. Dr. Paquin has placed a number of patients on this plan of treatment exclusively, and has so far succeeded about as well as by hypodermic medication.

From numerous observations, Dr. Paquin is thoroughly convinced that the serums of various kinds that produce these effects are not necessarily infectious. The preservative (four per cent. trikresol) is sufficient to prevent the development of germs in the bottled serum. Of course, serum accidentally contaminated by air germs or germs introduced by the syringe might, at the proper time, be capable of developing a culture in a test-tube; but such quantity of air-germs is usually harmless. Possibly a simple local abscess—such as occurs sometimes after morphine injections—may follow one of a thousand injections of serum. Not a drop of serum leaves the laboratory that does not come from an output that has been tested both microscopically and bacteriologically, and also in small animals and human beings. In fact, it seems impossible for the Paquin serums to be contaminated, because they are gathered with the greatest possible aseptic precautions, which exclude every germ, as above explained.

Of the millions of injections made of serums of all kinds, there have not been over a half-dozen deaths reported from this form of medication; and at least three of these deaths appeared to be due to some other cause. So, after all, the untoward symptoms mentioned as sometimes occurring after serum injections, are more alarming than dangerous—although, of course, they must in time be eliminated.

Experiments conducted with antistreptococcus serum and antitubercle serum combined, and alternately, have given most gratifying results: and Dr. Paquin suggests that hereafter, in cases of mixed infection, we use the antistreptococcus serum, as, after pus has disappeared, the field for the action of tubercle antitoxin is clearer, and the results so far obtained are most encouraging.

It will thus be seen that no effort is made by Dr. Paquin to deny that alarming immediate effects may follow the hypodermic use of antitubercle serum; but up to the present time no fatality nor permanent injury has resulted. It is right also to state for Dr. Paquin that his antitubercle serum should not be classed as a "secret remedy."

But suppose that here and there a fatality followed its use, is that any reason why we should not continue its administration until something better can be found, provided clinical observations and experiences prove it a valuable adjuvant to other established treatments of tubercular phthisis? What would we think of the surgeon who would deny to his patient a resort to laparotomy, under judicious circumstances, simply because some deaths are clearly attributable to the operation itself? Serum therapy is in its infancy. Its possibilities are already well proven in the wonderful records of cures of diphtheria by antitoxin. That serum seems already to have been perfected. Antitubercle serum is, as yet, not so thoroughly perfected: but progress is being constantly made in the direction of a perfected product. In its present state, no one recommends it as a substitute for other agents, proven to be useful, but it is a valuable addition to the therapy of tuberculosis.

During the past twelve months, the speaker has treated some 23 cases of pulmonary tuberculosis—many of the cases being at the Virginia Hospital, where most of the doctors present have seen the patients. Of the number, he has had three deaths: but each of these patients was so far advanced in the last stage of the disease—as recognized by the doctors who referred the cases—that there was no hope even of materially prolonging their lives. The cases were hopeless from the moment they first came under treatment; and there is one other such case on whom the antitubercle serum treatment is being used rather as a satisfaction to the wishes of the patient than because there is any hope that it or anything else can save him.

This leaves 19 cases that are now living—3, or perhaps 4, are thoroughly recovered so far as any evidence of disease is concerned: 6 or 7 are so nearly recovered that they think themselves well, and, at their own risk, have practically discontinued serum treatment; 1 case has been lost sight of, as he went to Washington city, where the serum treatment was continued, and then moved to Texas in an improved state of health: in 3 or 4 cases, the disease seems to be held in check, but without perceptible advancement toward a cure: the remaining cases are under daily treatment, and it is too soon to formulate an exact prognosis, except as to one case that is evidently fast recovering. Creosote (beechwood) in capsules, by constant inhalation, in the form of Hancock's compound creosote mixture, etc., are among the chief agents employed in treatment beside the hypodermics of antitubercle serum. Of course, all else that is found helpful in individual cases is adopted. Until antitubercle serum was begun, a year ago, the other agents he now employs were used; but no such general good resulted to so many. With such a record, then, the speaker is thoroughly convinced that in Paquin's antitubercle serum we have a valuable adjuvant in the treatment of pulmonary tuberculosis; and although he has patients who now and then show the unpleasant, even alarming effects, spoken of, he sees no more reason for discontinuing the use of the serum treatment than would the surgeon feel with reference to an approved operation, because now and then he has to deal with an unusually serious, bloody or painful one.

Dr. O. R. Robins has used antitubercle serum in two cases. One being almost moribund when first seen, no improvement could be expected. The second case had had tuberculosis six months when he was called to attend it. There had been hæmorrhages, the left lung was consolidated, and evening temperature 101.4°. The first injection was five minims of the serum, and it was increased five minims daily until twenty-five were reached. At this time, the patient was on the fourth bottle, and the cough has diminished, sputum has become frothy and light, as in bronchitis, and the temperature has been normal for the past four weeks. There were no bad symptoms from the treatment, except swelling and pain of the axillary glands before the first bottle was finished. Pain has disappeared. The patient did not increase in weight. All collateral treatment was cut off.

Dr. Gordon, replying to Dr. Edwards said, in the case he reported, the fever was disproportionate, and when the serum was discontinued, it fell, rising again as on its resumption. When stopped the second time, the patient began to improve, and has continued to do so. Referring to Dr. Paquin's statement as to harm resulting from even simple saline solutions, Dr. Gordon said a pint or a quart could be injected without effect, and that it was done in every day practice. Regarding Dr. Edwards' cases, it must be remembered that he has given other remedies in the accepted line of antitubercular treatment, and the question is: "How far the improvements were due to them?"

ABSTRACTS.

CONTRIBUTION TO THE ETIOLOGY OF RETENTION OF URINE.

W. Knopfelmacher (Cen. f. Kind) says only a few etiological factors are usually given in the reports on the retention of urine in children. Biedert recognizes only one—ischuria spastica; Teevan only three mechanical causes—congenital stricture of the meatus, phimosis, and stone. English mentions:

1. Abdominal developments and defects.
2. Congenital narrowing of the urethra.
3. Ridges formed by the mucous membrane of the urethra.
4. Excess of uric acid, gravel and stone.
5. Inflammatory processes of the kidneys, the bladder, the prostate gland, the urethra, the foreskin (oftentimes due to smegma, to periprostatitis, and pericystitis, originating from the vessels of the navel, or from coxitis).
6. Retention cysts in the follicles of the urethra, closure of the sinus pocularis in boys, swelling of the papillæ.
7. Changes in the prostate (congenital hypertrophies).
8. Foreign bodies and growths in the urinary organs. Bokai adds:
9. Sagging of the mucous membrane of the bladder.
10. Strictures.
11. Local atony and paresis of the bladder.
12. Stupor in infectious diseases.
13. Diseases of the brain and spinal cord.

It is remarkable that in this etiological enumeration, there is an absence of just that group, which in adults so frequently causes retention of urine, viz.: neoplasm in the neighboring organs. The following observations will prove that even in infants this etiological cause must be taken into consideration.

A child aged 9 months was brought into the clinic with the history of a continued fever rising to 40° C., of having had no passage for some weeks, and had not passed water for 50 hours. Examination revealed an evenly distended abdomen, and in the median line over the symphyses an oval, elastic tumor, which reached to the umbilicus—the distended bladder. Percussion was dull over the region of the bladder and in both flanks; over the rest of the abdomen percussion was very tympanitic; no fluctuation was felt. Normal dulness of liver and spleen was much diminished on account of the presence of meteorism. The urine was turbid, slightly ammoniacal, contained a small quantity of albumin, and many pus cells were found in its sediment. Catheterization was easy. The finger was easily introduced into the rectum, but when passed backward came in contact with great resistance, which seemed to fill the whole pelvis, and encroached so far on the symphysis pubis, that the finger could just be moved within the rectum. On pressing upward fecal masses were felt. Believing the retention to be due to an accumulation of fecal matter, and its pressure through the rectum, a high irrigation of the bowel with water and oil was ordered. The child then passed a little urine, but the bladder could not be fully emptied. After the child had been in the hospital four days, the fever continuing, and other symptoms remaining unchanged, it died.

An autopsy revealed, besides a pyelonephritis suppurative acuta, the presence of a tumor which was of the size and shape of a hen's egg, attached to the lower sacral vertebra and pressing the rectum forward, at the same time compressing it and the neck of the bladder. The extraordinarily congested, dark red swelling proved to be a round-celled sarcoma of the sacrum.

This case is not an isolated on. There is on record the case of H. Smith, in which a fibroid of the pelvis caused retention of urine in a child 19 months old, and another by Jones, of a 12-year-old boy, caused by a lymphadenoma of the lesser pelvis. The reason that tumors of the pelvis do not more frequently cause retention is, probably, because tumors found in this region (the pelvis), and which are always congenital in infants, have a tendency to grow outward.

To complete the etiology of retention in children, two more causes must be cited: The atresia hymenalis, where the accumulation of secretions may produce compression of the urethra, and the tumors of the sexual organs in girls; for example, the polyp sarcomas of the vagina, which produce retention partly from compression of the urethra, and partly by extension onto the urethra or wall of the bladder.—Pediatrics.

EARLY SIGNS OF LOCOMOTOR ATAXIA.

According to Professor Fournier, the first symptoms of locomotor ataxy may be classed as follows: 1. Sign of Westphal. 2. Sign of Romberg. 3. The "stairs" sign. 4. Crossing of the legs. 5. Walking at the word of command. 6. Standing on one leg.

1. Westphal's sign is well known. It consists in the abolition of the patellar tendon reflex, and is present in two-thirds of the cases.

2. Romberg's sign can be thus appreciated: The eye is an indirect regulator of motion. It helps to correct deviations in walking and maintains the equilibrium. When a patient is suspected of incipient ataxy, it will often suffice to make him close his eyes when in the erect position to verify the diagnosis. In a few instances his body will oscillate, and if the malady is somewhat advanced he will be in danger of falling.

3. The "stairs" symptom. One of the first and most constant symptoms of incipient locomotor ataxy is the difficulty with which the patient will descend stairs. If questioned closely on the subject, he will say that at the very outset of his malady he was always afraid of falling when coming down stairs.

4. The manner in which a patient crosses his legs is often significant. In the normal state, a man when performing that act lifts one leg simply to the height necessary to pass it over the other, whereas in the affection under consideration he lifts it much higher than necessary, describing a large segment of a circle.

5. Walking at the word of command. The patient seated is told to get up and walk instantly. After rising he will hesitate, as if he wanted to find his equilibrium before starting off. If while in motion he is told to stop short, his body, obeying the impulsion, inclines forward as if about to salute, or, on the contrary, jerks himself backward in order to resist the impulsion forward.

6. The patient is asked to stand on one leg, at first with his eyes open, afterward closed. Although man is not made for this position, yet he can balance himself pretty firmly for a little while. The ataxic will experience a great deal of difficulty, and will instinctively call to his aid his other foot, so as not to fall. If his eyes are closed he will not be able to stand one instant, and if not held would fall heavily to the ground.

Such are the symptoms of the incipient locomotor ataxy. They will not be all present frequently, but they should be all sought for in order to avoid an error which might have grave consequences.—The Practitioner.

GONORRHOEA IN WOMEN.

By BERNARD GORDON, M.D.

"Nitrate of silver, although very popular at present in the treatment of gonorrhœa, will probably soon be displaced by either one of the two new remedies—argentamin and argonin. They answer more to the requirements of an ideal remedy for gonorrhœa; namely, while they destroy the gonococcus and penetrate deeply into the tissues, still they do no harm to them."

"Argonin: A soluble silver-caseine salt. It is a fine, white powder, soluble in hot water; ammonia increases its solubility. It is a non-coagulating antiseptic, like argentamin. Argonin is strongly recommended by Jadassohn, of Breslau, R. Meyer and Schaeffer in 1.5 to 2 per cent. solutions. This remedy, combining the good properties of argentamin with its additional advantage of being non-irritating in character, will probably become in time the most popular antigonorrhœicum."

In speaking of Bartholinitis gonorrhœica the author says: "Treatment depends upon circumstances. Either dilate the canal by a sound and apply antiseptics, such as argonin, argentamin, nitrate of silver, etc., or incise, if there is an abscess. Should the glandular body be affected, extirpation becomes the most rational treatment."

"Should I meet an obstinate case of gonorrhœal cystitis, I would try argonin or argentamin."

"Ureteritis, when detected cystoscopically, must not be neglected, or it may cost the patient's life. Check it immediately by a local application, by Kelly method, of either nitrate of silver two per cent.; argentamin, 1 to 2,000; or argonin, one per cent. The last may be the safest and most reliable gonococcicide. For a short time I have been using a solution of argonin, two per cent., instead of the nitrate of silver, in my cases of vulvo vaginitis."

(Read before the New York Academy of Medicine, Section on Obstetrics and Gynecology, October 22, 1896.—Reported in Medical Record.)

Abstract of clinical lecture delivered at the New York School of Clinical Medicine, November 25, 1896. GOTTHEIL.

A careful consideration and trial of the various methods of treating the syphilodermata has led me to the following conclusion:

1. In the primary stage, when only the chancre is present, no general treatment; calomel locally.

2. As soon as the secondary period sets in, as shown by the general adenopathy, angina, cephalalgia, and eruption, the internal treatment for mild cases should be $\frac{1}{4}$ to $\frac{3}{4}$ of a grain of the proto-iodide of mercury t. d., continued for three months, or until the symptoms disappear. In severer cases, with pustular eruptions, severe anginas, persistent headaches, etc., a course of 6 to 10 intra-muscular injections of 10 per cent. calomel-albolene suspension, 5 to 10 minims at intervals of 5 to 15 days, should be employed.

3. After completion of the course and cessation of the symptoms, employ tonics, etc., without specific treatment, for three months.

4. Thereupon a second calomel course as above, plus a small dose

(15 grains) of iodide of potassium in milk after meals. This to be given whether later secondary symptoms of the skin and mucosæ appear or not.

5. Second intermission of treatment, lasting three to six months, according to the presence or absence of symptoms.

In the second year, if tertiary lesions marked by deeper and more localized ulceration are present, give the iodide of potassium increasing doses (60 to 600 grains) daily, as may be necessary. Combine with it occasional courses of calomel injections. If no lesions appear, give a mild course of both.

The best local treatment of the syphilodermata is with the mercurial plaster-mull.

THERAPEUTICS.

COD-LIVER OIL EMULSION.

By PROFESSOR GRAY.*

Cod-liver oil emulsions, the use of which has become general, have their origin in the old form of emulsive syrups with a cod-liver oil basis, but the objection which caused the latter to fall into disuse is avoided. These syrups, although satisfactorily disguising the taste and smell, only contained about S.5 per cent. of oil. Emulsions as now prepared are intended to disguise the appearance, taste, and smell of the oil, and to contain as a rule 50 per cent. of the active ingredient; they also serve for administering such remedies as phosphates, hypophosphites, arseniates, creosote, eucalyptol, etc., when their association with cod-liver oil is desired. Numerous processes have been proposed for the preparation of cod-liver oil emulsion. The main object of this paper is to discover a formula which can be used extemporaneously, and, in addition, fulfil other necessary conditions. We will first discuss the conditions which should be fulfilled by a formula for an emulsion as we now understand it.

1. Simplicity; no Special Form of Apparatus being Needed.—All commercial formulæ have their good points. It has been shown that a prolonged and energetic pounding—which is ensured by using mechanical beaters, or Merier's beater with its three-jointed pestles—ensures the stability of the emulsion by favoring the perfect division of the fatty body, but a pharmacist's working formula should only require the use of a mortar.

2. Use of the Usual and Unobjectionable Emulsifying Agents.—The most popular agents can be grouped as follows:

A. Usual Emulsifying Agents.

Gum Acacia.

Gum Tragacanth.—Acacia emulsions are more stable if tragacanth is used as well.

Gum and Starch.—This combination is useless.

Glycerine.

Glycerine and Gum.

Gelatine (white gelatine or Irish moss).

Yolk of Egg.

Condensed Milk.

Saponine.

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* Translated from the Repertoire de Pharmacie. Through Ph. Jour. and Trans.

B. Digestive Ferments or Their Products.

Malt Extract.—The preparation of the U. S. Pharmacopœia should not be used, as it is only the dextrine and maltose contained in this extract which are of service, the diastase being of no use at all.

Pancreatine.

Peptone.

C. Alkaline Substances.

Sodium Carbonate.

Lime Water.

Glycerine and Lime (prescribed by many practitioners).

Saccharated Lime.

With regard to the above classifying agents the use of saponine or quillaia does not seem free from danger; malt extract, condensed milk, and other agents are not generally used, whilst alkaline substances answer in special cases. I prefer to use gums.

3. Stability of the Emulsion.—Absolute stability is impossible; even the best makes require to be shaken before taking, still separation should be slow, and brisk agitation should cause perfect re-emulsification. This result is easily arrived at by means of gums.

4. Greater Strength.—Fifty per cent. is the usual standard. This leaves a sufficient margin for adulteration in the emulsifying agent, and, if necessary, in the other remedies.

5. Suitable Correctives.—Although it is difficult to disguise the odor, the taste is more easily masked, but the acrid sensation in the throat is seldom entirely suppressed. Two classes of correctives are used:

A. Sweetening Correctives.

Sugar, glycerine, and various syrups have often been combined with the oil with good results. Stout has recommended ammoniated licorice, using 50 to 60 grammes to each liter of oil. Saccharine may, perhaps, be looked upon as the most powerful corrective. Its intense sweetness overcomes the taste of the oil. The proportion used varies from 0.05 to 0.2 gramme per liter. M. Eisenchitz has proposed to dissolve it in its own weight of acetic ether previous to introducing it into oil. But as the question arises whether it is advisable to give saccharine in long-repeated doses, especially to children or to invalids whose digestive organs are weak, it is, perhaps, better to confine ourselves to sugar and licorice.

B. Aromatic Correctives.

The combination of various essences with the oil is a well-known plan; bitter almond, citron, neroli, eucalyptus, mint, canella, wintergreen, aniseed, sassafras, vanilla and roasted coffee are looked upon as the most efficacious, whether used alone or variously combined. The use of a perfumed spirit is, in my opinion, still more efficacious, cognac, rum, and kirsch can be recommended. Robinson has proposed using a 10-per-cent. solution of chloroform in alcohol in the proportion of 4 grammes to 30 of oil, whilst Foster recommends 10 drops of ether to each tablespoonful of oil. Care should be taken not to use essences excessively, as they are apt to fatigue the stomach.

6. Sufficiently Lengthy Preservation.—To guarantee indefinite keeping properties, as manufacturers are in the habit of doing, is useless, as such a condition of things can only be arrived at by the excessive addition of preservatives, such as glycerine, spirits, and essences. The use of glycerine is recommended by many authors and specialists in the quadruple capacity of emulsifying agent, by reasons of its viscosity, and of sweetening, preservative, and analeptic agent. The last effort does not seem certain, seeing that the nutrient properties of glycerine have been disputed. With regard to its emulsifying power, is not its viscosity balanced by its high density? I prefer to exclude it from a general formula. It will suffice for our purpose, since the preparation is made extemporaneously, if its keeping qualities can be guaranteed for the time necessary to use up such quantities as the pharmacist may be able to sell, say, one or two liters.

The formula which I propose fulfils the foregoing requirements satisfactorily, and the simplicity of the process in particular excels all others. Two forms of it have been worked out in response to the different tastes of patients and physicians; first, a creamy emulsion having a mixture of acacia and tragacanth as the emulsifying base, and a stable consistency which only allows of a very slight aqueous separation at the bottom, even after standing for a long time; secondly, a liquid emulsion, having a basis of acacia and liquid consistence, which soon separates into two layers, easily mixed by being shaken when taken.

A. Creamy Cod-liver Oil Emulsion.

Cod-liver oil	500	grammes.
Finely sifted sugar	190	"
Powdered gum acacia	5	"
Powdered gum tragacanth	5	"
Coffee infusion	200	"
Rum or kirsch	100	"

Mix the sugar and gums in a mortar; weigh the oil and cold coffee infusion into the bottle which will contain the emulsion, and shake until mixed. Pour into the mortar, at the same time constantly stirring, sufficient of the mixture to make a semi-liquid; weigh the spirit and add to it the mixture of oil and coffee remaining in the bottle; shake and mix with the emulsion by degrees.

B. Liquid Cod-Liver Oil Emulsion.

The creamy emulsion can be modified so as to form the liquid emulsion by replacing the tragacanth by the same quantity of acacia. The corrective chosen in these typical formulæ imparts a color of café au lait to the emulsions, in which the odor and especially the taste are conveniently disguised. But in this particular the formula can be modified ad lib. Various remedies can also be introduced.

7. Change of Correctives.—First, the spirit can be omitted and the quantity of coffee infusion increased to 300 grammes; in this case the coffee no longer suffices to cover the taste of the oil, and it becomes necessary to such an essence as bitter almonds dissolved in the oil to the extent of 20 or 30 drops per kilogramme. Secondly, one of the essences previously mentioned can be substituted for the coffee itself, by dissolving previously in the cod-liver oil; the emulsion of coffee is then replaced by water or an aromatic water. Thirdly, a more intense sweetness is obtained by mixing 30 grammes of glycerine with the aqueous vehicle.

8. Addition of Other Remedies.—First, in the aqueous vehicle, hypophosphite of sodium or calcium, phospho-glycerate of sodium or calcium (in the last instance, the alcohol had better be omitted), alkaline, iodide, sodium arseniate, soluble salts of iron, etc., can be dissolved. Hypophosphites and iodides cause a disagreeable taste, in addition to that of the oil; the addition of a strongly flavored corrective is then necessary; such as bitter almond, alone or associated with coffee, coffee, or kirsch, eucalyptus essence, etc.

Secondly, 300 grammes of lime-water can be substituted for the aqueous vehicle; in this case the liquid emulsion formula will suffice, the lime-water itself having an emulsive action, which makes up for the absence of the tragacanth; still the stability of the emulsion will be increased by mixing the oil and lime-water in the mortar instead of the bottle, the water being gradually mixed with the oil by trituration. A strong aromatic corrective will still be necessary.

Thirdly, such remedies as creosote, guaiacol, eucalyptol, iodine, mercuric iodide, iodoform, etc., which are insoluble in water, can be dissolved in the cod-liver oil if the extent of the solubilities be remembered. In support of the foregoing I will give, in conclusion, an example of an emulsion to which commonly occurring remedies have been added:

Creamy Emulsion of Cod-Liver Oil and Hypophosphites.

Cod-liver oil....................	500	grammes.
Essence of bitter almonds.........	20	drops.
Essence of wintergreen............	20	"
Finely sifted sugar................	190	grammes.
Powdered acacia...................	5	"
Powdered tragacanth...............	5	"
Distilled water....................	285	"

Calcium hypophosphite	10 grammes.
Sodium hypophosphite..........	5 "

Dissolve the essences in the oil, the salts in the water, and proceed as before.—The New England Druggist.

AN OINTMENT FOR MUMPS.

According to the Journal des Praticiens, May 9, 1896, Tranchet recommends the following:

Ichthyol;
Chloride of ammonium, 30 grains;
Lard, 1 ounce.

This ointment is to be applied to the swollen parts three times a day. In some instances vaseline may be used in place of the lard, and sometimes belladonna may be added with advantage.

From the British Medical Journal (Journal of the British Medical Association).—October 17, 1896.

Ladevie (Allgem. Wien med. Zeitung, Sept. 1st and 8th, 1896) records a large number of observations made by himself and others upon the antiseptic and therapeutic properties of Traumatol. It appears to have been used with great success in the treatment of varicose ulcers, eczema, metritis, gonorrhœal vaginitis, soft chancers, and wounds both infected and surgical. Bacteriological researches also prove its antiseptic power. The author states that in contrast to iodoform, which is both irritating and poisonous, traumatol is absolutely harmless and non-irritating both locally and generally, properties which he considers sufficient to give it a high rank in the long list of antiseptics. Internally, its antiseptic action on the respiratory tract is as potent as that of creosote or iodoform. Furthermore, it exerts a most favorable influence upon that ordinary intractable complaint, tuberculosis diarrhœa, a property which is said to be shared by no other drug hitherto tried for the purpose.

RECTAL ALIMENTATION.

The following prescriptions are given by the Journal des Praticiens:
As a nutritive enema:

Cod-liver oil........................	5 ounces
Yolk of one egg.	
Lime water.........................	10 ounces

This mixture is sufficient for four or five enemata, which may be given during the course of a day. In other instances the following may be employed:

Cod-liver oil	5 ounces
Yolk of one egg.	
Chloride of sodium	40 grains
Water	10 ounces

This is also sufficient for four or five separate enemata.

In still other cases, where it is desirable to use the hypophosphites, and the stomach will not retain them, the following rectal injection may be employed:

Cod-liver oil	1 pint
Gum tragacanth	35 grains
Gum arabic	1½ ounces
Hypophosphite of lime	35 grains
Lime water, a sufficient quantity to make 1 quart.	

From four to six ounces of this liquid may be used as an injection.

For dysentery:

Sulphate of quinine	10 grains
Powdered ipecac root	5 grains
Chloride of ammonium	10 grains
Tincture of opium	12 drops
Distilled water	1 ounce

Of this a small teaspoonful may be taken every four hours.—Am. Druggist.

A PROMINENT AUXILIARY IN INFANT FEEDING.

The numerous articles constantly appearing on the feeding of infants not only exemplify the great importance of this subject but they also demonstrate that authorities are not yet agreed as to what constitutes the best method of artificial nutrition for infants who for some reason or other are unable to nurse at the breast. Some years ago Dr. Rieth suggested that by adding albumoses to cow's milk it could be made to resemble human milk as closely as possible. This idea was further elaborated by Goldman, who recommended the addition of Somatose, a practically pure albumoses product, to diluted cow's milk, together with cream and sugar of milk. Milk prepared according to this formula has been considerably used in Germany and with excellent results. In cases where this mixture is, however, not available, a good substitute may be secured by diluting cow's milk with equal parts of a Somatose solution (30 grains to a quart of the mixture) and adding about 6 drachms of cane sugar, as recently recommended by Dr. F. Schramm (Archiv. f. Kinderheilkunde, Bd. xxi. To older children he gave 30 to 45 grains daily in soups or milk. The nine cases reported by this author comprising infants suffering from malnutrition, acute dyspepsia, gastro-intestinal catarrh, acute febrile diseases, or convalescing from acute disorders illustrate most excellently the high nutritive value of Somatose. Under its use in these small doses the children, although greatly reduced in nutrition, promptly increased in weight—the best test of its ability to make good the waste of the albuminous elements of the body. In acute febrile affections Somatose acted not only as a nutriment, but as a tonic, as the little patients made a remarkably rapid recovery. Dr. Schramm enumerated the following advantages of this preparation as an infant food: 1. Somatose consists of nine-tenths of albumoses. 2. It contains no elements incapable of absorption or capable of fermentation. 3. It contains the potassium phosphate so important in the formation of cellular tissue. 4. It is readily digested and almost tasteless and odorless. 5. It produces a fine flocculent coagulation of cow's milk caseine. 6. It is well tolerated even by a sensitive stomach and absorbed even by a damaged epithelium. The author therefore considers Somatose a valuable addition to the list of dietetic remedies for children and a prominent auxiliary in the artificial nutrition of infants.

CLINICAL RECORD.

SOME IDEAS ON ALTERATIVES.

By H. G. REEMSNYDER, M.D., Ephrate, Pa.

The word "alterative" is construed by a great many medical writers as a term that can be applied to any drug or remedy of whose therapeutics not much is known, and less written. Consequently our materia medica is full of so-called alteratives, many of which are seldom used and possess an action far different from that ascribed to them, but for want of a better term are thus denominated.

According to Brunton, an alterative is a drug that improves the general condition of the body, stimulates nutrition, and regulates the waste elimination without exerting any perceptible action on individual organs.

This clearly defines the therapeutic action of those drugs called alteratives, a study of which is always interesting since it is to them that much of the success in the treatment of chronic diseases is due.

The knowledge of the fact that a great many chronic diseases and conditions are amenable to treatment with benefit by alteratives is not by any means confined to the profession of medicine, but it has reached the laity and has spread through all the length and breadth of the land, as is evidenced by the general use of what are known as "blood purifiers," some of which have an enormous sale.

It is, therefore, essential to the physician to be acquainted with the drugs which are thus employed, as there is scarcely a condition of a chronic nature in which a tonic alterative will not be of benefit. Every physician must have noted that there are always a certain number of patients among his clientele who may be called "regulars," who require a course of medical treatment about once every month or two, in order to preserve their peace of mind, if not that of their body; who come with a train of symptoms, as varied and as remarkable as they themselves are, and which are withal so vague and incomplete that no earthly physician can diagnose their troubles, who, nevertheless, will be greatly benefited by an alterative which will improve the nutrition, eliminate waste, and not effect any individual organ.

Without doubt our best alteratives are to be found in the vegetable kingdom, and these have the advantage of being free from harm in prolonged administration, which cannot always be said of the indiscriminate use of such powerful mineral substances as mercury or arsenic. Another advantage can be attained in the use of vegetable remedies in the preparation of the green drug, upon which point I believe almost all authorities agree.

There has always been, however, on the part of our manufacturing chemists, somewhat of a tendency to ignore the demand for green drug extracts, so that it has become almost impossible to obtain reliable extracts, which are made from the green drug. However, we have one preparation of this character that experience has proved reliable, and which for certain therapeutic action is unsurpassed. I have reference to that known as iodia, which is composed of the active principles obtained from the green roots of stillingia, helonias, saxifraga, and menispermum, to which are added ferri phosphatum and potassium iodide. In theory this preparation presents an ideal formula, while in practice it produces certain beneficial results.

In addition to those cases mentioned as being susceptible to marked benefit by the use of an alterative such as here indicated, this preparation has another and perhaps more important action, that of certainlv curing syphilis. I have had ample opportunity to test this remedy in cases of syphilis in all stages, and I am in a position to positively assert that in this disease we can find nothing which gives more promising effects, and which allows us to give more encouragement to the unfortunate victims.

I report a few cases that have come under my care a year or more ago, were treated with iodia, and have remained free from the disease to the present time :

Case I.—Male: age, forty-four; married; had contracted syphilis about three years before consulting me. He gave a clear history of primary sore, followed by the usual symptoms, and was treated by another physician at that time. He had possession of the prescriptions which he had been using and they were shown to me. Large doses of corrosive sublimate had been prescribed, with arsenic iodide and sarsaparilla. He seemed to improve rapidly, and after a time discontinued treatment. He consulted me for relief from a bursa of the right elbow, which he had been treating with external applications, without relief, of course. In inquiring into his history I asked him whether he ever had syphilis. "Three years ago," he replied, "but that's all right now.". I told him plainly that his present trouble undoubtedly was caused by the disease which he had contracted at that time, and that doubtless other signs of the disease could be found. Inquiry further revealed dull pains over the region of the liver, much dull headache, and a peculiar scaly condition of the nails.

I promptly told him that he would require active and persistent treatment to overcome his trouble; explained to him that there was internal indication of syphilis, which would sooner or later be the cause of much mischief if not corrected. With the understanding that he would continue six months at least, I put him on the iodia, one teaspoonful three times a day, gradually increasing to one and a half teaspoonfuls. I furnished him with a month's supply, and did not see him until some thirty days after his first visit when he returned to my office. The bursa had almost entirely disappeared, and he said he was feeling very much better "all through" since he had been taking the medicine. I gave him another month's supply and did not see him again until the medicine had been taken. No vestige of the bursa remained, and as he was feeling very well, it was with the greatest difficulty that I persuaded him of the necessity for continuing the treatment. He, however, did as I requested, and at the end of six months I allowed him to cease treatment and await developments. In an interview with him a few weeks ago, eleven months after treatment was stopped, he reported himself entirely free from anything which could be ascribed to syphilis.

Case II.—Female: age, thirty-one; occupation, servant. Consulted me with the following history: Severe dysmenorrhœa, itching sensation all over the body, dull headaches most of the time, leucorrhœa occasionally, and a general feeling of malaise and despondency. Told me that another physician had treated her for syphilis, which she had contracted several years before, at the age of twenty-six, and she was intelligent enough to know the virulence of this disease, and ascribe her ailments to it. Treatment with iodia gave marked benefit, and in due time effected a cure, the dysmenorrhœa ceasing to be so severe at her first menstruation following treatment, with a total relief lasting over a year, to the present time.

Cases of this kind could be multiplied, but rough outlines are only given to acquaint the reader who may not have given this remedy a trial of its good results. I have found it especially valuable in cases requiring a uterine tonic and general tonic. Those stubborn cases of leucorrhœa, ovarian pains, bearing down sensations, etc., will be found, in most cases, where there is no organic trouble, to respond to this treatment. In certain skin diseases, where there is a tendency to scaly formations, itching or persistent ulcers, with suitable local treatment the remedy will be found a most valuable adjunct.—Med. & Surg. Reporter, Jan. 16, 1897.

MISCELLANEOUS.

THE NEW YORK CITY BOARD OF HEALTH AND TUBERCULOUS DISEASE.—The following communication dated January 11, has been sent by Dr. Hermann M. Biggs, pathologist and director of the board's bacteriological laboratories, and Dr. T. Mitchell Prudden, the board's consulting pathologist, to the Hon. Charles G. Wilson, the president of the board, and has been approved by the board:

Nearly eight years ago the Health Department of the city of New York took its first steps in the education of the people regarding the communicable and preventable character of pulmonary tuberculosis. At that time this view of the nature of tuberculosis was held by only a very small proportion of the medical profession, but also a widespread belief among the laity, that pulmonary tuberculosis results solely from the transmission of infectious material from the sick to the well, and is wholly preventable.

It is a little more than three years since the board determined that the time had arrived when more energetic measures for the prevention of this disease could be properly taken, and adopted resolutions requiring the notification of all cases occurring in public institutions in this city; the inspection of premises in tenement house districts, the instruction of the occupants, when reported as suffering from tuberculosis, and the renovation, when required, of all rooms or apartments occupied by consumptives which had been vacated by death or removal. The board also at this time offered to assist physicians, free of charge, in the diagnosis of cases of suspected tuberculosis by the microscopical examination of the sputum.

The action of the Health Department in relation to this disease has always been as nearly abreast of the knowledge furnished by the most advanced scientific investigations as was practicable, and the methods adopted in the past have been followed by the most encouraging results.

The department has cause for extreme gratification in the marked increase in intelligence and care, especially among the poor, which is observed in the presence of this disease, as the direct outcome of its conservative and educational policy, and in the proportionate curtailment of the danger and the suffering incident to it. During the past twelve years there has been a reduction in the general mortality from all tuberculous diseases of more than thirty per cent. in New York city.

But the time has now arrived, we believe, when more comprehensive and radical measures should be adopted to rapidly and materially diminish the prevalence of pulmonary tuberculosis in this city.

Some idea of the enormous sanitary importance of the subject is obtained by reference to the records of the department showing the reported cases and deaths during the past year. Nearly 9,000 cases of tuberculosis were reported to this department, and nearly 6,000 deaths resulted from this disease. It is conservatively estimated that at least 20,000 cases of well-developed and recognized pulmonary tuberculosis now exist in this city, and an additional large number of obscure and incipient forms of the disease. A very large proportion of the former cases constitute more or less dangerous centers for infection, the degree of danger depending, in each instance, upon the intelligence and care which are exercised in the destruction of the expectoration. It may be safely assumed that from the failure to safely dispose of the sputum of consumptives, from thirty to fifty inhabitants of this city daily become infected by tuberculosis, and of these about one-half later die from the disease. All this suffering and death, in view of modern scientific knowledge, we know to be largely preventable by the efficient enforcement of simple, well-understood and easily applied methods of cleanliness, disinfection, and isolation.

The knowledge now at command regarding the methods of extension of pulmonary tuberculosis entirely justifies the belief that its ravages can as certainly be limited by proper sanitary control and appropriate treatment as can other infectious diseases, more acute, more dramatic, and more readily communicated, but at the same time far less prevalent, less fatal, and incomparably less important to the welfare of the community. We fully believe that with proper regulations tuberculosis may be restricted within the narrowest bounds and eventually, perhaps, almost exterminated. This is not the idle dream of sanitary enthusiasts or theorists, but is a conviction founded upon the most thorough and conclusive experimental investigations, which have been amply confirmed by practical experience.

In order to make possible such restriction in the prevalence of this disease, it is necessary that the Health Department shall assume a more complete and comprehensive control. This requires, first, the adoption of such measures as shall make possible the general sanitary supervision of pulmonary tuberculosis under well-defined conditions and regulations, differing in many respects from those applied to other more readily communicable diseases; second, the possession of such facilities for the care of the poor suffering from it as shall make possible the removal, when necessary, of those who are dangerous sources of infection.

From the beginning of this work the officials of this department have encountered, in the utter lack of proper facilities for the care of consumptives, an obstacle to practical success so great and so disheartening that we feel impelled to urge our conviction that the grave responsibilities which rest upon the Health Department in this matter cannot longer be adequately sustained without the immediate establishment, under its direct control, of a hospital for the care and treatment of this disease. No week passes in which the officials of this department do not encounter instances in which the members of many households, numerous inmates of crowded tenement houses, employes in dusty and unventilated workshops and many others, are exposed to imminent peril from victims of this disease, to whom the doors of our overcrowded public institutions are closed, or who reject all proffered assistance and instruction, and, from ignorance, indifference, or inability through weakness due to the disease, scatter infectious material broadcast, and thus diminish their own chances for recovery and imperil the health and safety of others. In such cases the sanitary suggestions of the Health-Department inspectors are now futile, and effective action impossible. We are convinced that no other factor is so potent to-day in perpetuating that ominous death-list from pulmonary tuberculosis as the lack of proper facilities for the care of the poor of this city stricken with this malady.

* * *

REVIEWS.

Swedish Movements; or, Medical Gymnastics. By Dr. T. J. Hartelius, Knight of the North Star Order, etc. Translated by A. B. Olsen, M.D., with introduction and notes by J. H. Kellogg, M.D., life member of the British Gynæcological Society, etc. Published by the Modern Medicine Publishing Company, Battle Creek, Mich. 1896.

Transactions of the Medical Society of the State of New York, for the year 1896. Published by the Society. 1896.

Artificial Anæsthesia and Manual of Anæsthetic Agents and Their Employment in the Treatment of Disease. By Laurence Turnbull, M.D., Ph.G.; aural surgeon of the Jefferson Medical College Hospital, etc. Fourth edition, revised and enlarged, with illustrations. Philadelphia: P. Blakiston, Son & Co., 1012 Walnut street. 1896.

This fourth edition of "Artificial Anæsthesia" was, curiously enough, as averred by the author, issued upon the fifteenth anniversary of the discovery of ether. The book has for a long time been perfectly familiar to all who are interested in the question of anæsthesia, and the present edition simply brings up to date all the advances made in anæsthetics, local and otherwise. The table on deaths from anæsthesia—both ether and chloroform—has been "obtained with much labor and trouble from all the reliable sources within reach," and the result of the whole study is simply to confirm the opinion first formed by the author that "Ether is the safest systemic anæsthetic to be employed." In looking over this book we are again reminded of the immense part played by the dentists in the question of practical anæsthesia, as well the matter of instruments as in the matter of drugs. It will be a surprise to many physicians who read the work to find the immense number of drugs that have been used and recommended as local anæsthetics. Not as much is said as could have been said in regard to the use of oxygen, with both chloroform and ether; but when a book has reached its fourth edition, the question of its utility and desirability cannot be raised.

The Diseases of Infancy and Childhood; for the Use of Students and Practitioners of Medicine. By L. Emmett Holt, A.M., M.D., Professor of Diseases of Children in the New York Polyclinic, etc. With two hundred and four illustrations, including seven colored plates. New York: D. Appleton & Co. 1897.

One of the first offerings of the year, and one which is quite in keeping with our earliest associations, is the book whose title we have just quoted. Its author has for some years been known, not only as a valuable teacher, but as an invaluable contributor to our best periodicals; and we are not surprised to see that he has collected in a single volume the results of most of his periodical work, and added thereto an immense amount of labor in order to produce an extensive, able and valuable treatise on the subject with which he has been specially indentified. Upon theoretical grounds it was easy to question the advisability of so soon following with an individual treatise the "Systems" so lately published on this subject in this country. Practically, whenever a man has something worth hearing to tell, or a better way of putting what is already known, no other excuse is needed for his taking copy to the publishers. The author, in his preface, says: "The rapid advance made during the past few years in this department of medicine, is sufficient justification, if one were needed, for another general work on the diseases of infancy and childhood. It is not claimed that the present work is a complete one, for completeness in so broad a subject in a single volume is impossible. The pathology and symptomatology of disease in children who have passed their seventh or eighth year, really differ little from those of adolescence and young adults. It is in infancy and early childhood only that the peculiar conditions exist which separate pædiatrics from general medicine, and entitle it to be ranked as a special department. These stages, therefore, are chiefly devoted to the subjects of the nutrition and the diseases of infants and young children. The discussion of questions relating to operative surgery has been purposely omitted. What is said regarding surgical diseases has been from the standpoint of the physician, not that of the surgeon, and relates chiefly to symptoms and early diagnosis. Rather more space than is usual in a clinical work has been given to pathology, and the discussion of lesion; my reasons for this being, first, that most of the processes which are peculiar to very early life have received but scant attention in works on pathology; secondly, such knowledge is absolutely indispensable to the correct understanding of these diseases clinically; and, thirdly, because I have been fortunate in having rather exceptional opportunities for post-mortem study in connection with my clinical work." The arrangement of the work is into two parts. The first consists of only thirty pages, divided into three chapters; one on the hygiene and the general care of infants and young children; one on the growth and development of the body; and one on the peculiarities of disease in children. The second part is divided into sections, ten altogether—the first, on diseases of the newly born; the second, on nutrition, and this section occupies almost one hundred pages, or about one-tenth of the whole work; nor is this, we believe, at all out of proportion to the importance of this subject. Section three, diseases of the digestive system, takes up about one-fifth of the whole work, and here again the author has shown wise discrimination in giving to the one great source of all infantile troubles a large amount of his space and attention. Then follows sections on diseases of the respiratory system, of the circulatory system, of the uro-genital system, of the nervous system, of the blood, lymph nodes, bones, and so on. A separate section considers the specific infectious diseases, and rheumatism and diabetes mellitus, under the head of "Other General Diseases" complete the book, as well as section ten. The illustrations throughout are profuse, original in a large measure, and excellent. The typographical work needs no comment, except that it is done by the publishers, whose names are given.

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EDITORIALS.

THE PHYSICIAN AND POLITICS.

At the recent semi-centennial celebration of the founding of the New York Academy of Medicine, the President of the United States, in the course of a charmingly simple, easy and natural address, presented in a more forcible light than we have hitherto had occasion to consider it, the relation of the physician to public offices, the public, and politics. The question of medical politics we do not wish to touch upon, nor do we wish our readers to attach any wrong interpretation to the use of the words. The natural impulse of the medical man who is an earnest devotee to his profession, is most decidedly against the idea of an invasion of public places and the appearance in public offices, for the simple reason that such a condition of affairs is generally interpreted to mean that his interest in politics is greater than his interest in medicine. Not only so, but the kind of work that a physician constantly is called upon to perform is calculated to put him entirely out of sympathy with both methods and people with whom he would come in contact in political life. And yet we must confess that the able presentation of the claims of the state and the country upon every educated individual, as presented in the address mentioned, seem to demand, even where a personal sacrifice is involved, the active interest of the medical man in the practical politics of his town, city, state and country. We are constantly complaining because so little attention is given to the warning and to the request of such a large and intelligent part of the public as our medical fraternity. Is it not for this very reason, namely that the physicians themselves do not act as a unit and do not take, as a rule, any interest whatever in the ordinary course of politics? This being the case, from the standpoint of the politician, they can have no weight. From the standpoint of the public, a large portion of which is organized and managed by the politician, it cannot compel a following. It does seem that the solution of the situation is comparatively simple. The physicians themselves must follow more closely, not only what affects them, but also whatever is of interest to their own municipality. In short, they must identify themselves at the outset with what appears to them to be the measures most intelligent and best adapted to the needs of the community at large; and they must do this, not to the injury of their professional work, nor to the loss of opportunity to increase their professional information. On the other hand, the public must come to view such a changed relation in a very different light from that in which it now does. It must realize that an important factor for good has entered into political life. It must appreciate that in so doing this factor loses none of its claims to the respect and confidence of individuals in the special work to which its life is dedicated. The gist of the whole matter is simply this, that in a republic it is incumbent upon every citizen to be a politician.

RECENT REGULATIONS TO PREVENT THE SPREAD OF PULMONARY TUBERCULOSIS.

The Health Board of New York city, which is ever thoughtful and provident for the health of the community in general and its own bailiwick in particular, has stirred up a considerable amount of discussion by the profession as well as by the laity, as the result of the latest recommendations which they have succeeded in having appoved, advising an amendment to the sanitary code declaring that tuberculosis be officially considered a communicable disease, etc. (We print elsewhere the communication on the subject.) Every week a new writer complains to one or more of the medical journals of some disagreeable feature which would result from such an enactment. The latest complaint was because the family were given minute instructions by the Board of Health in regard to the disposal of the dead body, in a case, which, at the worst, could have been only in a very early stage of phthisis. The communicability of consumption has been established beyond a doubt, but the uncertain and not always accurate pathological examinations do not enable us to diagnose with certainty every case of the disease. But granting that such a diagnosis be possible, a vigorous protest is apt to be entered by private patients, especially in early stages of phthisis, against having their names entered upon the Board of Health register as sources of communicable disease. On the other hand, the physicians themselves are apt to take more personal interest in the prevention of the dissemination of tuberculosis by their patients, and in order to do so they must inform themselves, as well as their patients, in regard to the means to be pursued. We believe that an immense amount of good will be accomplished by the recommendations, in that it will compel the physicians to inform themselves fully in regard to the most modern methods for the prevention of the spread of tuberculosis; but we question both the propriety and the right of the Health Board, or any other board, to compel such information as they propose to demand from the physicians respecting every case of phthisis. Fortunately for the profession in New York city, the board consists of a number of most intelligent and able, active practitioners, and whatever legislation emanates from it affects the interests of the individual members of the board equally with those of every practitioner in this city. This is, doubtless, the reason there has always been such a cordial relation between the profession and this part of the public service. The board has always shown itself ready and willing to assist, not only the public, but its professional brethren in whatever seemed to contribute either to good of the public at large or to the individual patient; and this is why we are not a little anxious in regard to this most recent proposition.

ACTION OF THE MED. ASSOC. OF THE DISTRICT OF COLUMBIA.

Apropos of our editorial last month on the abuse of medical charity the following recommendations, adopted by the Medical Association of the District of Columbia, February 16, 1897, will be of interest to the profession, as well as a guide in the matters upon which they touch:

I. That every institution for medical charity shall require from every applicant for relief in a Hospital or Dispensary a written certificate to be obtained as hereinafter provided. Emergency cases are to be excepted from the operation of this rule.

II. That such certificate be obtained from Physicians to the Poor, the Board of Associated Charities and any registered physician.

III. That cases of sick and injured persons found upon the streets, in the stations or elsewhere, who require immediate treatment shall be carried to the Emergency Hospital, or the nearest Hospital having an Emergency Service, or to their homes, if so directed by the patient or his friends.

IV. That Emergency Patients shall not be detained longer in such Institutions than the necessity of the case imperatively demands, but shall be discharged from the service and sent to their homes or to some public hospital as the patient may elect.

V. That members of this Association shall be entitled to the privilege of attending private patients occupying private rooms in any of the public hospitals of this city.

VI. That in future the members of the Medical Staff of Hospitals when attending medical or surgical cases in private pay-rooms shall insist upon proper payment for their services except in the case of such patients who are clearly unable to pay for same.

VII. That whenever the Medical Staff or a majority thereof of a Hospital or Dispensary resigns, and, when after due hearing, this Association finds that the resignations were for just and sufficient cause, it shall be forbidden for any member of this Association to accept a position on the Staff of said Hospital or Dispensary.

VIII. That whenever one or more members of the Medical Staff of a Hospital or Dispensary are dismissed, and when after due investigation this Association finds that such dismissal was without just and sufficient cause, it shall be forbidden for any member of this Association to fill the vacancy created thereby.

IX. That complaints made under Rules VII. and VIII. shall be made in writing to the Standing Committee, which, after due consideration, shall report its findings to the Association.

J. R. Wellington, M.D., W. P. Carr, M.D.,
Secretary. *President.*

We shall have occasion later to consider separately some of these recommendations.

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ORIGINAL ARTICLES.

TUBERCULAR MENINGITIS.*

By J. ALLISON HODGES, M.D., Richmond, Va.,

Professor of Nervous and Mental Diseases, University College of Medicine, Richmond, Va.

From whatever standpoint viewed, tubercular meningitis is a most interesting disease.

The consideration of its bacillary origin, its peculiar mode of invasion, its stubborn resistance to all known therapeutic measures, its appalling record of mortality being the most directly fatal of any disease of early childhood; either or all are questions inviting the interest and enthusiasm of every student of disease.

It may almost be called an incurable disease; for, notwithstanding three cases of its happy termination are reported, it is not at all improbable that an incorrect diagnosis was a source of error even with those distinguished clinicians.

Discarding for the present other interesting and instructive features, I desire especially to direct attention to its diagnosis, for I consider this of paramount importance, in not so much that its fatal termination can be avoided, as it is at present treated, but that the ends of scientific exactness in this as in other diseases may be attained, and that future errors of prognosis may be avoided.

I am persuaded from personal practice, and from my experience as consultant, that many cases of this disease have been incorrectly interpreted by the attending physician, and the significance of many of its varied symptoms overlooked, until its unfortunate termination revealed the correct nature of the illness. A melancholy experience with two such cases is a lesson still indelibly impressed upon my mind, and it was this that first attracted my attention to my lamentable error in diagnosis; for, deceived by the mild train of symptoms before coma supervened, I overlooked the danger that was impending, and gave an encouraging prognosis! When the symptoms that shall now be detailed, are considered, is it possible that others may realize that they too may have to plead guilty to such a charge of mistaken diagnosis?

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* Read before the Richmond Academy of Medicine and Surgery, January 26, 1897.

Tubercular meningitis presents a preponderance of nervous symptoms, and is a distinctively bacillary disease, being peculiar to childhood, and usually attacking children between two and ten years of age. The membranes involved are the same as those in simple meningitis, but the anatomical lesions differ, for the latter affects the pia and arachnoid on the convexity of the brain, while a meningitis of tubercular character attacks those membranes at the base.

The prodromal symptoms are vague, and for this reason are apt to be misleading. As much as any clinician, I abhor the division of any disease into arbitrary stages, being well aware how variable are the manifestations, but for the sake of clearness, I prefer to so divide the symptoms of this disease; for, as a rule, there is a regular sequence in them; and it is only from a contemplation of this succession of symptoms, when associated with other manifestations, that the cause of the ominous changes which occur in the later periods is suspected and can be diagnosed.

I believe that too great importance cannot be attached to the mode of invasion of this disease as a diagnostic point. In simple meningitis there is a stormy onset, and a convulsion ushers in the scene, while in tubercular meningitis the convulsion is usually one of the last acts in the sad drama. In tubercular meningitis, this period of invasion is so peculiar that it cannot be mistaken by the careful observer, if the facts which are of such import in the determination of this particular disease are faithfully elicited.

The initiatory symptoms are so indefinite as often to be deceptive. The child is not at once seized with a sudden illness. There is simply a gradual failing of the general health, and the only physical sign of disease is a more or less evident emaciation. This change in the condition of the child also is a variable one, being rapid and striking in some, while in others it is scarcely noticeable. In all cases, however, the emaciation at last becomes a marked feature of the case.

During the progress of the change another symptom, which may be termed a moral change, develops itself, since it is manifested in both the habits and temper of the child: for the little patient, according to its individual temperament, may exhibit all the phases and variations of its nature. Formerly bright and playful, it now becomes morose and sluggish, or irritable and disagreeable. This change of disposition is often supposed to be a reflex condition, due to some primary cause of disease in childhood, such as dentition or some digestive disturbance; but if carefully investigated, no such causative factor is usually discovered.

This moral change in the usual life of the child is not an evanescent one, but is continuously and progressively more marked, and its development should convey a deeper significance to the experienced practitioner.

First Stage.—Generally coincident with the moral change another symptom supervenes which is significant, namely, the existence of a headache. This cephalalgia in these cases is characteristic, for it is violent, paroxysmal and generally localized, as evidenced by the actions of the little sufferer. Vomiting is usually associated with it, and it is not the ordinary vomiting from gastric disturbances, but the true cerebral vomiting, propulsive or regurgitative in character, unaccompanied by pain or nausea, as, for instance, is the vomiting observed in hepatic or abdominal disorders. These symptoms, accompanied by a mild degree of pyrexia, the temperature, most usually exceeding 100½° F., a slightly accelerated pulse, wakefulness, photophobia, constipation, contraction of the pupils, with possibly some hyperæsthesia of the abdomen, constitute what I would denominate the first stage. In proportion to the intensity of this disease in individual cases will the tardy or rapid succession of the symptoms noted above be manifested, for, generally in affections of the brain, symptoms of irritation and excitation appear first; but sometimes the symptoms of profound depression are initiatory, and this regular succession of phenomena is not so prominent. These prodromal symptoms of the first stage may extend over a period ranging from one to several weeks.

Second Stage.—The second stage of the disease (always supposing there is a regular succession of phenomena) ushers in more pronounced symptoms, and there appears to be an increase in their intensity. The temperature rises slightly, and a peculiar embarrassment of respiration is noticeable, the patient sighing deeply and long. The pulse now becomes remarkably slow, and there is generally an interference in its rhythm which corresponds to the change in the respiration. The eyes half close, and the eyeballs move slowly in a lateral direction, one or both pupils dilating. At this period there are sudden flushes of the face, and a kind of mottling appears upon the skin, over which, if the finger is drawn, red marks are left temporarily; and a sunken, retracted condition of the abdomen is evident. While these two conditions are characteristic in this affection, they are not pathognomonic. During this stage there is also developed a cephalic cry, which once heard is never forgotten. The remission in the fever, which is apt to occur now, is often deceptive and may lead to false hopes. Increased somnolence becomes apparent towards the close of this stage, and in reality is but the beginning of the end.

Third Stage.—The foregoing train of symptoms is but introductory to the last stage, where coma is the prominent feature, overshadowing all the other conditions. Evidences of vital depression and of organic implication are now more marked, and slight twitchings around the mouth and eyelids, or a convulsion, followed by ptosis, strabismus, or paralysis of the face or extremities promptly follow. The head may become retracted, and all the other symptoms increase in severity till the fatal termination ensues, after a period of three or four weeks.

Not all cases pursue this definite course, for in some children the symptoms may be much milder, while in others there may be so much somnolence from the beginning as to obscure many of them. In fact, to say anything positive about the course of this disease is impossible; it is sometimes acute, sometimes chronic, sometimes presenting long intermissions, and sometimes steadily progressive. Even the manner of onset may vary much, and the different stages or periods so coalesce as to be scarcely distinguishable from each other.

Differential Diagnosis.—From the foregoing, it is evident that an absolutely positive diagnosis of tubercular meningitis can be made only when a full and true history of the successive phenomena of the illness can be obtained; or when the symptoms of a tubercular meningitis supervene upon tubercular lesions that have previously existed in other organs. Some of the diseases with which tubercular meningitis may be confounded are:

(1.) Simple meningitis. In this disease, the prodromal symptoms are wanting; the history of tuberculous infection or predisposition is lacking; the onset is more sudden, and the course is more quickly run.

(2.) Cerebro-spinal meningitis. When this disease occurs endomically or sporadically, the vaso-motor disturbances occur earlier, and the characteristic eruption is apt to be present.

(3.) The hydrocephaloid state. This condition in many particulars is so similar to a tubercular meningitis that it can be differentiated only by a history of some gastro-enteric disturbance prior to the supervention of the brain symptoms.

(4.) Malarial fever. The history of malarial infection, or the proper administration of quinine, usually solves the question of diagnosis; otherwise, an examination of the blood must be made for the specific germ.

(5.) Typhoid fever. In this disease, the symptoms of a cerebral character very closely simulate a tubercular meningitis, especially if the temperature curve be atypical, as is frequently the case. A microscopical examination of the blood is often necessary as a crucial test, but, clinically, it is always well to remember that in typhoid there is no peculiar or rhythmical disturbance of the respiration, as in meningitis from a tuberculous infection.

Treatment.—When the prognosis of this disease can be summed up in one word, death, it is evident that so far no treatment has ever been successful. The multiplicity of remedies suggested corresponds only with their inefficiency.

Recognizing personally the utter helplessness of all known measures of therapeusis, as employed in the past, to combat this fatal malady, I purpose in the future to use anti-tubercle serum, or if that proves inefficient, to employ surgical means to remove the intra-cranial pressure. I am aware that such a procedure would be radicalism in the extreme, but it is a principle which is applied to all other forms of internal pressure and engorgement, and the necessities of such cases demand the application of any measures based on scientific warrant. The ventricles of the brain have been drained and flushed with antiseptic solutions in other diseases, then why not in this? Surely, the phenomenal successes secured in some cases by simply flushing the peritoneum for tubercular disease ought to be encouraging and stimulating to like endeavors in these cases.

In the treatment of this disease, the past is a history of successive and multiplied failures as regard the use of medicines. Is it a vain hope, then, that in the future surgery may solve the mystery of its successful treatment?

ACUTE LOBAR PNEUMONIA—TREATMENT SPECIALLY.*

By RAMON D. GARCIN, M.D., Richmond, Va.,
Instructor in Practice of Medicine, Medical College of Virginia.

Prognosis.—Osler makes the statement, in his classical work, that "Pneumonia is one of the most fatal of acute diseases. Hospital statistics show that the mortality ranges from 20 to 40 per cent. Of 1,012 cases treated in the Montreal General Hospital, the mortality was 20.4 per cent. It appears to be somewhat more fatal in Southern climates. Statistics from the Charity Hospital, New Orleans, show the death-rate to be 28.01 per cent. It has been urged that the mortality in this disease has been steadily increasing, and attempts have been made to connect this increase with the expectant plan of treatment, but careful analysis of 1,000 cases by Townsend, in the Massachusetts General Hospital, indicates clearly that, when all circumstances are taken into consideration, the conclusion is not justified. He found that when all fatal cases over fifty years of age were omitted, and those patients who were delicate, intemperate, or the subject of some complication, there was very little variation from decade to decade, and that, excluding these cases, the rate was little over 10 per cent. In answer to the assertion that the modified treatment is in part responsible for the increased mortality, he shows clearly that the rise in death-rate took place in the period prior to 1860, when the treatment was entirely or in great part heroic."

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* Read before the Richmond Academy of Medicine and Surgery, February 9, 1897.

In a short interview with a prominent member of the profession, he made the statement to me that in a discussion of the mortality of acute pneumonia with Dr. Hunter McGuire, their experience with this disease was similiar. They had never known a young adult, whose previous history was good, to die with pneumonia. A careful study of my notes of cases shows that, while I have treated a good many cases, in healthy young adults, my limited experience has been the same as theirs.

Diagnosis.—"No disease is more readily recognized in a large majority of cases. The external character, the sputa, and the physical signs combine to make one of the clearest of clinical pictures."—*Osler.* Dr. Osler says: "After a study, in the post-mortem room, of my own and others' mistakes, I think that the ordinary lobar pneumonia of adults is rarely overlooked. Judging from my autopsy records, I should say that errors are particularly liable to occur in the intercurrent pneumonias, in those complicating chronic affections, and in the disease as met in children, the aged and drunkards. Pleurisy, with effusion, is, I believe, not often mistaken, except in children."

Treatment.—Wood and Fitz: "In the treatment of pneumonia, it is essential to recognize that, though the disease may be a unit from the pathological point of view, therapeutically it comprises essentially diverse diseases. A pneumonia whose physical signs cannot be made out in the beginning, but gradually creep up towards the chest wall, a pneumonia whose expectoration is in the beginning prune juice, whose crepitant rale is never typical, whose physical signs are obscure until complete consolidation gives percussion dulness, or a pneumonia occurring in the alcoholic, in the old, in the victim of renal disease, in the broken-down debauchee, is, in its management, essentially distinct from a pneumonia the result of exposure of a strong, healthy countryman to a western blizzard or other cold." "When, in the first twenty-four hours of a pneumonia, there is violent constitutional reaction, with flushed face, rapid and noisy breathing, bloody sputa, intense headache and drowsiness, a hard, bounding, or a tense, corded pulse, venesection may markedly lessen all the symptoms, and, if combined with dry cupping over the whole chest, may, we believe, lessen the amount of engorgement of the lung and the final area of consolidation." Wet compresses, and even the application of the ice-bag very freely over the affected lung, are recommended by some of the leading text books and authorities; and just at present, the ice treatment is quite a fad at the North. At the very beginning of an attack, where active congestion is just begun, ice might prove of service, as it is of value in any other form of active congestion; but, even then, it should be, it seems to me, used with the greatest caution, if at all, because of the shock, more or less, and the causes calling for its indication should have to be very carefully selected. Personally, I have had no experience with it, and shall certainly not use it till I am fully convinced that the after-effects are not worse that the remedy. "The question how far we should attempt to reduce temperature is important in the extreme, and is answered very differently in practice by different practitioners." Although a great many authorities advise against the use of any of the coal-tar class, my experience does not agree with their statement. When the temperature is 103°-4°, I use frequently five grain doses of phenacetine, repeated at regular intervals, until the fever is reduced to 100° or so, and never, as yet, have I seen any but the best results follow its administration. Quinine, if used, has to be employed in large doses, and rather does harm than good, because of the resulting nervous excitation. I prefer the bi-sulphate or muriate.

"To prevent exhaustion, by maintaining the forces of the patient, is the great object of the nursing in pneumonia. Absolute confinement in bed from the beginning, regulating the temperature of room 70° to 74°, feeding at regular intervals (two or three hours)—food should be simple, nutritious, and digestible; milk, animal broths, clam broth is very acceptable." Panopepton (Fairchild's) and peptonoids (Arlington Chem. Co.). "Alcohol in the beginning of sthenic pneumonia is injurious. In the advanced stages of the disease, it being regarded therapeutically as allied to an infectious fever, it is of service, employed judiciously." "In the advanced stages of a severe pneumonia, tinct. digitalis, 5 to 15 minims at intervals of 4 to 6 hours, acts well as a heart stimulant. Its effects upon the pulse should be the guide for administering it."

Nitro-glycerine and amyl nitrite should be used with caution in sufficient doses to produce effects. They always lower the arterial pressure by depressing directly the muscle fibers in the blood-vessel's walls, and although their first action upon the heart is a stimulant, the slightest overdose converts such action into that of a powerful depressant. Carbonate of ammonium is very largely used, both as a stimulant and as an expectorant." Some authorities, among them one that I quote from very freely—Wood and Fitz—say, "there is no reason for believing that it has any effect on the consolidated lung, and that its power as a stimulant is certainly inferior." I must enter my humble protest against these assertions, for in several hundred cases of this disease I have treated in hospital and general practice, I can candidly say that I have found this drug of the utmost service, both as an expectorant and stimulant. My dosage is also at variance with theirs. They advise three grains or so; I employ five to ten grains, as indicated in the individual case.

Aromatic spirits of ammonia is also valuable as a cardiac stimulant when needed. The most important drug, in my hands, as a respiratory and circulating stimulant, is strychnine. "The special indication for the free use of this agent is cyanosis, with hurried breathing and other evidences of respiratory distress." "For relief of local pain, dry cups probably give quickest relief."

Blisters, the greatest single agent in treating this disease, have saved more lives than all the drugs in the materia medica. If used skilfully and when indicated, that is, in the beginning of the third stage, they will afford prompt relief from pain, aid the expectoration of the clogged lung and very materially hasten convalescence.

In conclusion, allow me, from the limited time assigned me, to entirely disclaim originality for this paper. The great mass of it is quoted from authorities—Osler, and Woods and Fitz. I have given them due credit for all their quotations, which are taken literally and liberally from their works.

2318 East Broad St., Richmond, Va.

SELECTIONS.

THE MANAGEMENT OF PNEUMONIA PATIENTS.*

By SIMON BARUCH, M.D., of New York.

My chief reason for bringing so trite a subject before you is that the fatality of pneumonia and the frequency of the discussion of its treatment indicate that it is very far from being settled in the minds of the profession. Another reason is a desire to summarize a personal experience extending over one-third of a century in city and country, private and hospital practice, and which represents a therapeutic evolution, a consideration of which may contribute an humble stone to that therapeutic structure which can be reared only by the aggregation of individual observations. It is hoped that the discussion of this subject to-night may furnish some clue to a better management of patients suffering from this very prevalent and fatal malady. The most effective therapeusis may be evolved only from the most thorough comprehension of the nature of disease. Whether stumbling upon it accidentally, as in the case of quinine for malarial diseases, or by a gradual evolution, as in the Brand bath for typhoid fever, or by a process of reasoning, as in the thyroid-gland treatment for myxedema, our therapeutic achievements are in proportion to our mastery of the nature of the disease.

The history of pneumonia illustrates this idea. I will not weary you with the recital of its historical stages, but confine myself to personal observations. The teaching of the schools and text-books in 1859-'62, and the practice in the military hospitals in which I served as interne, were clearly antiphlogistic, because the conception of lobar pneumonia at that time was a pure inflammation of the lung, whose tendency was to destroy life by apnœa (Watson). Delirium, pyrexia, and other threatening manifestations were regarded as "direct evidence that the pectoral mischief is telling through the circulation of venous blood through the brain." It was but a legitimate deduction from this view that the chief agents to bring about recovery were the so-called antiphlogistics, by which it was hoped to throttle the local disease. It would be a bootless task to review the contending opinions prevailing before and at the time mentioned.

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*Read before the New York Academy of Medicine, December 3, 1896.

Long before this period Dietl of Vienna had learned from the homœopaths (who were genuine Hahnemannians, and not the mongrel breed met with to-day) that pneumonia is a self-limited disease, tending to terminate favorably if the patient's vital forces be not despoiled by blood-letting, mercurials, and starvation.

Although he combated the prevailing "hematomania" with energy, he failed to make any permanent impression. In the first decennium of my active practice, Hughes Bennet and Austin Flint labored to inculcate similar doctrines. Their efforts were more successful, because the medical profession was beginning to emancipate itself from the thraldom into which that bloodthirsty monster phlogosis had cast them. In Vienna, a therapeutic nihilism attained ascendency, which, though not as fatal, did not fill the full measure of the physician's duty. Then came the quinine, aconite, and digitalis, and, later, the antipyretic era, from which the profession has not yet quite emancipated itself.

We stand to-day on the threshold of a view of croupous pneumonia which will contribute more to the saving of lives than all the doctrines and remedies suggested since the days of Hippocrates. The student is no longer taught, as I was taught, that the pneumonia patient dies from apnœa. Heart failure is the specter that now stands at the bedside of the patient in this and, for that matter, in most other acute diseases. But we are, fortunately, not lapsing into the Brunonian theory of asthenia, which demanded enormous quantities of alcoholic and other stimulants. To-day it is an almost universal axiom in the therapeutics of croupous pneumonia to prevent and combat heart failure. The view that this is an infectious disease, whose chief recognizable lesion is in the lungs, but whose lethal tendency is in the overwhelming of systemic force and vigor, is obtaining more and more recognition.

I would go further than this, even. Careful clinical observation and a judicious sifting of the testimony of reliable practitioners have brought us to the belief that croupous pneumonia is no more a lung disease than is typhoid fever a bowel disease. Whenever this doctrine shall receive universal acceptance, we will cease to treat the disease, and we shall be prepared for a more effective management of patients suffering from it.

It may be said that this idea is obtaining adherents in certain quarters. All of our recent text-books insist upon maintaining the patient's vital powers, as the main indication. That great American clinician, Austin Flint, read a paper before the State Medical Society in 1877, on "Pneumonic Fever," in which he referred to the analogy to typhoid fever. My interest being aroused, I discovered that, 200 years ago, Huxham had insisted upon the infectious character of pneumonia. Thus history has repeated itself in this as in other diseases, before the micro-organisms assigned to them had been discovered.

As to etiology: The present state of our knowledge may be briefly stated to be, that while cold may be a predisposing factor which diminishes the patient's resisting power, the Sternberg-Fraenkel diplococcus is an important factor in the production of croupous pneumonia, because it is present in about seventy-five per cent. of cases, although in some cases there are doubtless other organisms present. Sternberg and others have found the diplococcus in the mouths of healthy persons. Its mode of entrance into the lung and its pathogenic course are not yet clearly determined; its aspiration from the mouth seems to be claimed by most observers. The presence of some constitutional depression is a potent etiological factor. Age is a predisposing and exempting factor, inasmuch as young children are rarely attacked; while it is most prevalent among adults, and liability to it diminishes with advancing years.

Typhoid fever presents analogous conditions. The Eberth bacillus enters through the mouth and is lodged and developed in Peyer's glands. The chief differences are that the Eberth bacillus is not, like the diplococcus, capable of producing other diseases, and that the life period of the former is three to four weeks, while that of the latter is five to ten days. The analogy certainly holds with regard to the influence of depreciated constitution and age as etiological factors.

Analogy in Symptoms.—In pneumonia we have an increased leucocytosis, pyrexia, and more or less overwhelming of the nervous system, which is reflected to the heart, lungs, and other organs. The dyspnœa is, I believe, more the expression of a special toxemia than of the crippling of the lung, because it is not always in proportion to the extent of the lesion, and it disappears at the crisis, while the lung is still greatly obstructed by the exudates. In addition, we have cough, which of course, is due to the disturbance of the lungs and bronchi.

We have practically the same manifestations in typhoid fever, differing only in regard to the habitat of the micro-organism, diarrhœa taking the place of cough. The fever runs a different course because of the difference in the life-period of the organisms involved in each disease.

The most striking and important similarity is that the chief point of attack, in both diseases, is upon the nervous system; the toxemia resulting from the life and death of the micro-organisms spends its chief force upon the nerve centers. This explains the well-recognized clinical fact that the general symptoms are rarely a correct index to the local lesion. Dyspnœa and fever, especially, may be extreme, and the case may terminate fatally, with slight involvement of the lung, just as we may observe the most serious general condition in typhoid, without evidences of local lesion.

Crisis in croupous pneumonia is signalized by complete cessation of all the general symptoms, because the life-period of the micro-organisms is terminated. And yet the lung remains more or less solidified by exudates, as is evident from the physical signs continuing. So do the general symptoms of typhoid fever cease with the cessation of the life period of the Eberth bacillus, although the intestinal lesions are still so far from being removed that great care needs to be exercised with regard to diet, rest, and other preventive measures.

The prognosis of croupous pneumonia and typhoid fever bears close resemblance in many respects. The effect of previous mode of life, habits, and environment, before and during the illness, upon the issue of the case, is very much alike in both diseases, as is the influence of childhood upon the prognosis.

Having as briefly as possible in the time at my disposal traced the close analogy in the etiology, symptoms, and prognosis of these diseases, I propose to deduce from this analogy the expediency of following in pneumonia, as nearly as the differences indicated above may admit, the line of management which has afforded the best results in typhoid. We have learned from sad experience that the chief indication in typhoid fever is to enhance the patient's resisting capacity to the lethal agencies evolved by the infection-process.

It is my aim to emphasize a similar cause of action in pneumonia. The management of the pneumonic patient, which I propose to present to your consideration, is the result of an evolution from the methods, which some of you, like myself, have been taught, and which, doubtless, some of you, as I, have practised for years.

A distinct mental picture of my first case of croupous pneumonia rises before me. The patient was a young soldier, a hardy mountaineer, in whom a large portion of the right lung was consolidated. The chief surgeon of the hospital directed, as was the custom in 1862, flax-seed poultices to the chest, and a mercurial purgative, to be followed by nitrous powders, a preparation which is happily unknown to our recently graduated colleagues, consisting of 8 grains of nitrate of potassium with ½ grain of calomel and ⅕ grain of tartarized antimony, these powders to be administered every three hours until the gums were touched; i. e., until ptyalism ensued. Wet cups to the chest were added to emphasize the doctors' blows upon the enemy—inflammation of the lungs—with a view to deplete the latter. The inflammation was resolving beautifully when the left lung was attacked. The same treatment was resorted to, with the exception of a blister being substituted for the wet cupping as a concession to the evidently enfeebled condition of the patient. This lung also was beginning to resolve, but the vital powers had been so exhausted by the depleting measures, that the system was unable to furnish the necessary conditions for restoration. The result was that the disease was practically cured, but the patient died from sheer depletion, as evidenced by a necrotic gray deposit on the blistered surface.

This result of what was the classical treatment at that time (1862), made a deep impression on my youthful mind as a crucial test, and I say it with some satisfaction that no patient of mine has ever been salivated or cupped, and very few blistered since that time. I continued for a long time to commence the treatment with a mercurial purge, to give veratrum to reduce the pulse, to poultice the chest for comfort to the patient, and to give small doses of tartar emetic as an expectorant, and morphine to allay the cough. But the teachings of Hughes Bennet, Austin Flint, and Horatio C. Wood gradually weaned me from this mildly spoliative practice. Until a few years ago I felt as helpless in a case of pneumonia as I did at that time in typhoid fever. Recovery in both diseases seemed to be the result of conditions over which my control was feeble or lacking. The patient's constitution, the type of the disease, and his environments played the chief rôle. Treatment was symptomatic in both diseases—expectant it was termed—and not inaptly, for I always expected something to happen which I seemed powerless to prevent. When I learned the true principles and value of the Brand method in typhoid fever, a positive treatment was at my command by which, if called early, serious and fatal complications, against which I had formerly felt impotent, could be prevented. For the comfort and the sense of security derived from this method, since I mastered its technic, I shall ever be grateful to its author. And herein I was recently corroborated by Gilman Thompson, Delafield, Ball, and Northrup in the "Section of Practice."[1]

It has been my ardent desire to reach a similarly comforting attitude in pneumonia. Weary of the expectant plan, and observing its enormous fatality, especially in those depreciated by faulty modes of life and habits, I looked to hydrotherapy for succor. While I cannot say that the latter has been reached to an entirely satisfactory degree, observation and reflection have produced a method of management whose clinical value deserves to be tested further.

The prevailing type of the disease should be carefully considered. It has been demonstrated by careful observers that the vaunted success of certain methods of treatment may be attributed to a change of type in the disease and the previous conditions of the patient. Any new treatment should therefore be approached with caution and judgment.

Dr. Leaming, whom many of you remember as a practical, safe clinician, gives a graphic delineation of this idea in his little book published in 1884.

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[1] Med. Record, November 7, 1896.

"When I began my professional career, I believed, as I had been taught, that active interference was necessary in every step of this disease, to prevent destructive inflammation. But I soon found, and by accident, that cases would get well without active treatment; that the expectant plan and wise management was the best; simple medication, with stimulants and supporting alimentation. The mortality was so small that it seemed unnecessary for an uncomplicated case to reach a fatal result. When the fatal form of pneumonia began to occur in New York, I, with other physicians, was surprised at the failure of the managing method. The failure of the latter rendered a resort to the heroic method a necessity."

The type of pneumonia in this city appears to have remained as grave as it was in the time of which Leaming wrote. We still see and hear of cases dying rapidly. As recent sad illustrations in our own ranks may be cited the deaths of the lamented Loomis and of the brilliant young clinician Roosevelt. In hospital and private practice this disease is more fatal than any other acute disease of adults. In the New York city Board of Health statistics it ranks next to phthisis in fatality. I apprehend that the chief reason of this great fatality may be sought for and found in the helplessness of physicians in the presence of this fearful disease.

It follows, therefore, that the first consideration in the management of these cases is a due regard for the prevailing type.

Complete rest of body and mind, and good nursing, are essentials in furthering recovery. This cannot be too earnestly urged upon the patient and his family. The ventilation of the sick-room, too, which is much opposed by the laity, should be anxiously watched by the medical attendant, who is too prone to yield to existing prejudices, and thus to become an unwilling accomplice in depriving the patient of this greatest pabulum in infectious diseases—fresh, pure air. The patient and family should be positively assured that there is no danger of taking cold if the body temperature is elevated to any considerable extent. In summer and winter the air of the sick-room should be constantly renewed, even at the cost of reducing its temperature in winter to an extent which may be uncomfortable to the nurses. During the late war, cases of pneumonia treated in tents in rigorous weather did far better than those treated in improvised hospital buildings.

The diet should be restricted to milk and farinaceous broths, administered with precision, while the patient is awake, every two hours, in quantities ranging from four to eight ounces. I do not share the horror of asthenia in this disease, which is only too prevalent, and leads to undue feeding. The disease is of short duration; it notinfrequently suddenly attacks well-nourished persons in the midst of comparative good health. It seems an unphysiological proceeding to stuff these patients with concentrated foods, in large quantities, which they cannot assimilate, and thus to impose an additional tax upon the system. Stimulants, also, are rarely so necessary to the same extent as formerly, since the course of management here outlined was adopted. In alcoholic subjects whose consumption of liquor has been a daily habit and requirement, $\frac{1}{2}$ to 2 ounces of good whisky every two or three hours may be useful, especially while resolution is going on, but not as a spur to a failing heart, as is commonly the practice. I am not quite sure on this point, hence still feel myself under the dominion of this idea. It would seem that inasmuch as a large part of alcohol is eliminated through the lungs, the latter are needlessly overburdened while they are straining to the utmost their functionary capacity. I should like to hear the experience of the fellows of the Academy upon this point.

Of medicinal agents I use but very few. Besides an opening dose of 10 to 20 grains of calomel, strychnine, and an occasional dose of morphine for pain, are the only medicines I have found of any real value. When the first sound of the heart becomes muffled or feeble, hypodermics of strychnine, $\frac{1}{30}$ to $\frac{1}{10}$ gr., every three or four hours, offer a valuable aid to tide the patient over a trying period. Given even to the production of trismus, if necessary, it is far superior to digitalis or brandy. I do not fear an occasional dose of phenacetine when the patient is restless, with high temperature, but it should not become a routine practice for temperature reduction. It is rarely required. Quinine, aconite, veratrum, digitalis, and other drugs, I have discarded after a prolonged trial.

The administration of calomel requires some detail. It should be placed, as Leaming and Cammann direct, dry upon the tongue, and allowed to gradually disappear, being swallowed with the saliva, or, if necessary, washed down with a small quantity of water. These experienced practitioners tell us that "the inefficiency of the expectant method rendered a resort to the heroic a necessity, and with very encouraging results. Cases, such as had proved fatal by the mild treatment, were saved by the prompt exhibition of sedative doses of calomel, which are less depressing to the system than smaller doses." To-day we are able to give a more rational explanation of the action of calomel in pneumonia than that of a "sedative." Its effect is two-fold. First, it is a fact clearly established by Sternberg and others, that the diplococcus of Fraenkel, found most abundantly in the saliva, is destroyed by very feeble mercurial solutions. This may explain why the effect of calomel is so much more striking when allowed to slowly mingle with the saliva. Thus, the supply of micro-organisms is at once cut off, together with toxins which would continue to be evolved from these, so long as they are living. This is not an unreasonable explanation of the remarkably rapid change in the symptoms of even desperate cases which are reported by Leaming, and which I have observed.

Second, a large dose of calomel removes ptomaines that may have lodged in the gastro-intestinal canal. Calomel is the only agent to which investigations (made before guaiacol was discovered) have conceded internal antiseptic properties. Moreover, it clears the primæ viæ, as we older practitioners were taught to do; it clears the ship for action, as it were. It also disgorges the portal circulation, and probably thus relieves the stasis existing in the pulmonary circulation indirectly. Be the explanation what it may, clinical observation in typhoid fever, pneumonia, tonsilitis, diphtheria, and other infectious diseases has convinced me of its value when given in one large dose in the beginning of the case. It very rarely produces serious purgation. Even when diarrhœa exists, it does not salivate as readily as smaller doses, and it certainly requires no repetition. It is more easily retained by an irritable stomach than other medicines. It is my custom to direct the patient to rinse the mouth frequently with a saturated solution of potassium chlorate to insure against ptyalism, and to keep the mouth in a clean condition. Juergensen recommends weak mercurial solutions as a mouth-wash for the purpose of destroying the diplococci, whose favorite habitat is in the mouth.

Hydrotherapy, judiciously adapted to the indications of the case, has proven a valuable auxiliary in the management of these patients. In the croupous pneumonia of children, who are more easily handled, without great disturbance, the tub-bath is very useful when administered alongside of the bed, as in typhoid fever. The temperature should, however, rarely be below 85° F. Beginning with 95°, each bath may be reduced a few degrees, until the patient becomes accustomed to it, repeating every four hours until the temperature remains below 102.5°. Friction is a sine qua non, because chilling is more apt to occur in these patients than in typhoid fever, and for this reason the duration of the bath should be short—eight to ten minutes. The toxic agents which are operative in pneumonia appear to affect the heat-center quite differently from those evolved in typhoid fever. A bath of 80° F. often reduces the temperature in the former disease two to four degrees, while in the latter one degree would be a large reduction. Upon this clinical fact I have based a diagnostic bath which materially aids in the differentiation of typhoid fever from other diseases which so strongly resemble it during the first week. The value of the full bath in the pneumonia of children has been confirmed by the highest authorities, such as Penzoldt and Baginsky in Germany, Hutinel, of Paris, our own Jacobi, and others.

In adults, however, I have given up tub-baths, although very cold baths (50°—60° F.) have been lauded by Juergensen and commended (65°

F.) by Chief-Surgeon Vogl of Munich in a letter to myself. While they proved useful in many cases, I am convinced that the disturbance and agitation incident to a cold tub-bath increase the lung disturbance. If the patient is very dull, cyanotic, and presents great nerve prostration, resembling the typhoid state, I do not hesitate to choose between the two evils, an increase of the local disturbance and a decrease of nerve prostration, by plunging the patient into a bath of 80° F., or less, or seating him in water at 100° and pouring several basins of water at 65° to 75° F. over his head and shoulders. This is a valuable heart tonic. It serves, by deepening the inspirations, to dislodge accumulations in the bronchi, which have crippled the healthy as well as the affected lung.

The most useful hydriatric procedure in pneumonia is the wet compress. This consists of three folds of thin old linen stitched together at the edges, which has slits corresponding to the axillæ, so that it may be wrapped around the patient's chest snugly. The slits are made sufficiently deep to allow the upper edges of the compress to reach above the clavicles, and admit of the junction of the flaps thus formed on each side to cover the shoulders. Two such jackets, and two pieces of closely woven thin flannel of the same shape, but an inch wider and longer, should be provided and fitted to the patient. One of the former is rolled up and soaked in a basin of water at 60° F., and wrung out so that it remains quite damp without dripping. The flannel is now spread out upon an even surface and the wet compress put upon it, so that there remains an edge of flannel about an inch wide all around. Both are rolled together half way. While the patient is gently turned upon his left side, with the precaution of not allowing any exertion on his part, the compress is so placed upon the bed that the rolled part lies in close proximity to the left side of the patient, and the lower edge of the left slit is under the left axilla. Now the patient is quietly turned upon his back, so as to release the rolled-up portion. The latter is now unrolled, and both edges of the compress are brought forward upon the front of the chest and are thus made to envelop the latter snugly. The flannel cover, which has been allowed to lie upon the bed during the application of the wet compress, is now brought forward so as to quite cover the latter. It is secured by two safety-pins in front, and one upon each shoulder.

The well-known oiled silk jacket may serve as a model for the shape and manner of applying this wet compress. The latter should be changed every half-hour, unless the patient's temperature is below 102.5° F.; every hour unless it be 99.5° F., when it is discontinued. The removal is accomplished by preparing the second compress just like the first, paying attention to the water temperature being 60° F. When it and the flannel covering are rolled up in readiness, the first compress is removed and the second is applied. Thus a rotation is kept up every half-hour or hour as the case may require, night and day, unless the patient be asleep. The water in the basin should be renewed each time, and the compress rinsed off in another basin before it is rolled up for soaking, in order to insure thorough cleanliness and prevent furnuncles by furthering asepsis.

The technique of the procedure has been entered into with a detail that may seem needless. As will be shown later, these details insure precision, and upon their exact execution may depend success or failure. The physician should himself supervise the first application, just as he should supervise the first Brand bath in typhoid fever. A skilled nurse can apply these compresses with a minimum of disturbance.

There is need, too, of individualization. In the average case a temperature of 60° F. will be appropriate. Should the patient evince stupor or muttering delirium, a lower temperature should be adopted, and the chest should receive one or more dashes of colder water, before renewal of each compress. The same procedure is useful in broncho-pneumonia, when the bronchi are blocked by secretions, or cyanosis exists. In a case which I had the privilege of seeing with Dr. E. J. Ware, this application proved its worth. The patient had passed through an attack of typhoid fever lasting some ten weeks; her condition was so precarious that when I entered an adjoining room she was bidding her relatives adieu. By skilful management she had been sustained up to this time. The right lung in its entire posterior aspect was solidified. The temperature was usually below 102° F., and the pulse exceedingly rapid. There was no dyspnœa.

In this case the modified procedure referred to was extremely valuable. Inspiration soon deepened, the heart slowed, and in a few days the patient rallied from a most desperate condition.

A higher temperature than 60° F. may be used if there be much jactitation, insomnia, or excitability. In the latter event great benefit will accrue from allowing the compress to remain two hours and moistening it more thoroughly, whch converts the compress into a soothing fomentation that is not relaxing like a poultice.

In a colleague, whom I had the privilege of seeing with Dr. Palmer Cole, there was complete involvement of the entire right lung, with temperature ranging from 103° to 105° F., the heart action being fair. The patient being a morphine habitué, there was a decided neurotic element in the case, involving insomnia and great jactitation. The compresses were only applied hourly, despite the high temperature, but they were allowed to be quite moist, in order to obtain a calming effect. Although the left lung became involved later, Dr. Cole informed me that the patient recovered from his desperate condition, and that, in his opinion, his life was saved by the management here outlined. No remedial measure deserves adoption by the profession unless the rationale can be satisfactorily explained.

According to my view of pneumonia crouposa, the therapeutic indications are (1) to stimulate and invigorate the nerve centers with a view to enhancing the patient's vital powers, (2) to prevent and control heart failure, (3) to reduce temperature, (4) to eliminate toxins.

The nerve centers are well stimulated and rehabilitated by the repeated gentle shocks and subsequent reactive stimulation of the sensory fibers in the skin, both of which are conveyed to the central nervous system and thence reflected to the organs, upon whose functionating capacity depends the patient's ability to resist the toxic agencies circulating in the blood. We aim here to accomplish precisely the same object as with the Brand bath in typhoid fever. The milder form of shock is better adapted to pneumonia, because it is less disturbing, and because the toxemia, induced by the diplococcus, is less intense in the average case, and certainly has only one-third of the life period of the Eberth bacillus.

After a few compresses the patient grows more calm, the inspirations, which are deepened by each application, continue deeper, dyspnœa is markedly relieved, sleep ensues, appetite improves, and the skin and kidneys begin to act more freely. These clinical evidneces demonstrate the correctness of the rationale enunciated above.

The maintenance of the heart action is accomplished by the wet compresses in the following manner: When the cold compress is applied, there is a rapid contraction of the cutaneous vessels, which raises the tension at once, but gives way to a tonic dilatation of these vessels, which is evidenced by a ruddy hue of the skin. This dilatation differs very decidedly from that relaxed condition of the cutaneous vessels produced by warm poultices. The latter relax the vaso-constrictors, producing a paretic condition of the vessels, or a stasis, while cold applications stimulate the vaso-dilators, giving rise to an active dilatation, with maintenance of the tone of the vessels, an active hyperæmia, by reason of which the blood is propelled more vigorously through them. The heart is thus relieved; not by a vis-a-tergo, as is the case after digitalis, but by a vis-a-fronte, formed by broadening of the blood-stream in the cutaneous capillaries, whose enhanced tonicity aid at the same time in propelling the blood onward. Arterial tension is increased, as is evidenced by the better filling of the radial arteries. The right heart is indirectly aided by this enhancement of the general tone in the vascular apparatus, and may thus expend more force upon the pulmonary circulation, whose vessels contract more firmly by reason of the dilatation of the superficial vessels.

Romberg[1] has recently confirmed what I have, several years ago, and repeatedly since that time, insisted upon, that in acute infectious diseases we encounter disturbances of the circulation which manifest themselves clinically as reduced tension and diminished filling of the arteries, and which are commonly described as heart failure. Undoubtedly the condition of the peripheral vessels bears a very large share in the production of cardiac atony, as I have sought to impress when explaining the rationale of cold applications in typhoid fever and pneumonia.[2] Romberg has shown by experiments with injections into rabbits of Fraenkel's diplococci that the circulation is damaged by a paralysis of the vaso-motors, while the heart itself remains unaffected. It is true that the right ventricle is overloaded because of lung infiltration, but that this does not so seriously cripple the heart as is generally supposed is proved by the fact, referred to above, that when crisis ensues, the compression due to the exudates is not removed at once, and yet the respiration and circulation are relieved as if by magic. This can only be accounted for, if I may be allowed to reiterate so important a statement, by the nervous system being suddenly relieved of the toxic elements generated by the diplococcus, whose activity ceases at once when it has reached the end of its natural life-period. It is probable that crisis ensues when the antitoxins evolved in the natural course of the disease have attained an overbalance of power. The battle between the diplococcus, with its allies, and the patient's vital powers, is at an end. The most important effect of cold applications in pneumonia is, therefore, the aid and sustenance they furnish to the nervous system, which bears the brunt of the fight. The improvement of cardiac action is one of the results of this effect.

Reduction of temperature is an important therapeutic element in these cases. Persistent high temperature may enfeeble the heart, and certainly depresses the nerve centers. Some good clinicians, like Juergensen, claim that high temperature is the chief danger to patients suffering from pneumonia, and he advises in his latest work[3] that very cold baths, some below 60° F., are demanded to meet this "great danger." I shall not discuss this doctrine fully to-night. Since my argument against it before the New York State Medical Society in 1889, I have repeatedly expressed adverse views in this and other societies to this unfortunately too firmly established error, an error which has given unhappy prominence to the coal-tar antipyretics in the therapy of fevers. If any further testimony is needed to lay this ghost, which has so long affrighted timid practitioners, to rest, the discussion on fever in the Fourteenth Congress of Internal

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[1] Berlin. Klin. Wochenschrift, 1895, Nos. 51 and 52.

[2] "Uses of Water," vol. ii, p. 166.

[3] Penzoldt and Stinzing, "Allgemeine Therapie," 1896.

Medicine in April, 1896, demonstrates that we should cease to regard elevation of temperature as the cardinal symptom for therapeutic attack. While, however, these conclusions fully corroborate my own views expressed eight years ago, I may say that high temperature, though not actually and directly a lethal factor by inducing fatty degeneration of the cardiac muscular fiber, as claimed by Ziegler and others, does seriously cripple the work of the heart by imposing upon it more rapid action, and it does seriously interfere with the patient's comfort. High temperature, therefore, demands careful attention by measures that are not harmful like the routine use of coal-tar antipyretics. As I said elsewhere, I do not hesitate to use the latter occasionally when high temperature is associated with sleeplessness or great jactitation. They give great comfort to the patient, and are less annoying and disturbing than the wet compresses. But the latter are not used by me for their anti-thermic effect, which is only incidental.

The susceptibility of pneumonia patients to cold baths, and the great facility with which their temperature may be reduced, renders great care in the application of these measures imperative. For this reason I have abandoned cold baths, and have dwelt so minutely upon the method of application. That so mild a procedure as the wet compresses should be capable of reducing high temperatures in pneumonia, I have numerous charts to demonstrate. The decline is not as rapid as after cold baths, but it is pronounced and steady. It does not ensue after one or more compresses, nor does it follow the course of temperature reduction observed after cold baths in typhoid fever.

It decreases day by day one or more degrees. It is not the result of direct refrigeration; such an effect is neither aimed at nor obtainable. When a compress at 60° F. covered with flannel is applied around the chest of a pneumonia patient who presents a temperature of 102° to 106° F., there is an immediate cooling of the surface covered by it, which is followed by a gradual reaction with a more or less rapid rise of the surface temperature, until the latter is nearly the same as it was previous to the application. The surface is now bathed in a vapor produced by the heating of the compress. If the latter be allowed to remain sufficiently long, the flannel covering receives the vaporized water, and passes it outward until the compress becomes dry. But if the compress is changed as indicated above, the vapor is more slowly removed, and the skin and compress are found to be cooler than the flannel covering. Thus the gradual cooling process is continued until a fresh, cold compress is applied, when the skin, more sensitive by reason of having been bathed in this warm vapor, feels the shock more distinctly and reacts more fully. The error is not infrequently committed of covering a wet compress with oiled silk. While this would protect the bed and clothing of the patient, it would defeat the object of the compress by converting it into a poultice. If the body temperature is 103° F. or over, the compress applied as above becomes warm in half an hour, although evaporation of the moisture through the flannel renders the skin under it cooler than other parts of the body. When it is renewed, the repetition of the gentle shock and tonic dilatation occurs. Thus a slow cooling process, not relaxing or in any way depreciating, is maintained, which gradually lowers the general temperature, calms the patient, and contributes much to his recovery.

How far this cutaneous hyperæmia acts as a revulsive, it is impossible to ascertain, but it is a well-known fact that such hyperæmia is always accompanied by contraction of the blood-vessels in the parts underlying. Thus may a favorable effect be produced also upon the inflamed lung tissue, and its circulation be improved.

The elimination of toxins is promoted by the wet compress, as by all cold hydriatric procedures. It has been proved by Roque and Weil that the urotoxic coefficient of the urine trebled after cold baths in typhoid fever. All the secretions of the body are increased by cold applications. The alkalinity of the blood is also enhanced by the latter, as demonstrated by Alois Strasser. Tassinari (quoted by Winternitz) says that after septic infection of rabbits, the alkalinity of the blood is reduced about one-half. And it has been shown by others that in all infectious diseases the blood is less alkaline. Since alkalinity of the blood is a pre-requisite for the existence and activity of phagocytes, it is not unreasonable to assume that cold applications further the phagocytosis, and elimination of toxic products, by rendering the blood more alkaline. Hence Buchner is correct (as Winternitz claims) in asserting that by cold water treatment the powers of resistance to infectious agencies are enhanced.

This anti-toxic eliminating effect is increased by copious libations of water. It is my custom to administer in pneumonia, as in typhoid fever, 6 ounces of water every two hours, alternating with the same quantity of milk, night and day, when the patient is awake. The enormous increase of urine is scarcely credible. I have charts showing from 60 to 122 ounces in twenty-four hours. Some of the well-trained nurses, who have seen much hospital service, have volunteered an assurance of the correctness of their reports, under the impression that I would discredit large quantities.

There is a striking peculiarity impressed upon the regular course of croupous pneumonia by the method here outlined to which I desire to call attention. Although the fever, dyspnœa, and other distressing manifestations yield, to a remarkable extent, in a large proportion of cases, in one or two days, the physical signs do not change in a proportionate degree. Indeed, I have observed a distinct crisis in only about forty per cent. of the cases so treated; in the remainder, the disease ended by lysis of a very slow type. Improvement of the general symptoms goes on, the patient seems well, and is anxious to rise, but a dull percussion-note, muffled breath-sounds, and bronchial whisper bear testimony to lingering exudates.

As an example, I may cite the case of H. E., who sickened on December 8, 1895. His temperature ranged from 102° to 104° F. for four days, without any pronounced physical signs in the lungs, which were examined every day. On December 12th, consolidation of the entire posterior lower half of the right lung was made out. The mouth temperature was 104° F. at 3 P. M.; cough had been distressing several days; there were now rusty sputum and increased dyspnœa. The family being greatly alarmed, Dr. A. A. Smith was called in consultation, but did not see the patient until the next morning. Calomel having been administered, the wet compresses were now applied, with the effect of bringing the temperature down two degrees in five hours. During the night, the compresses were, by a misunderstanding of the nurse, renewed every hour. The patient sleeping soundly for the first time since his illness, complained to me of the frequent disturbance. After twelve compresses, the temperature was 101° F. at 8 A. M. When Dr. Smith saw him, at ten o'clock, he found him so comfortable that he expressed surprise at being called, and concurred in the treatment. Physical signs of consolidation were distinct. There had been no other treatment except 5 drops of dilute hydrochloric acid every two hours, as a placebo, followed by six ounces of water. Temperature reached normal point once every day for two days, and remained normal for ten days, when the patient was dismissed. Physical signs continued during Dr. Smith's attendance of five days, and dulness of the percussion-note had not entirely cleared up when I last saw him.

Clinical results confirm the rationale of this treatment, and attest its value. A quarter of a century ago the greatest living German clinician, Professor Niemeyer, said:[1] "I have made extensive employment of cold in the treatment of pneumonia, and, relying upon a large number of very favorable results, can recommend the procedure. In all cases, I cover the chest of the patient, and the affected side in particular, with cloths, which have been dipped in cold water, and well wrung out. . . . In the hospital at Prague, every pneumonia is treated with cold compresses and, according to the statement of Smoler, it is exceptional for a patient not to feel material relief from the treatment."

Why has a treatment which has received high commendation by so eminent a teacher not become an established practice in a disease which so often baffles the physician under other management?

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[1] "Text-book of Practical Medicine," vol. i, p. 185, translated by Humphreys and Hackly.

To the student of the history of hydrotherapy this question is not difficult. Although water is an orthodox remedy, to which Hippocrates devoted almost an entire book, and although it has been highly commended by eminent physicians at various times, it has not become the common property of the profession, because most of those who taught its use and value most earnestly failed to give definite directions for its application. Niemeyer illustrates this point. He simply advises "cloths wrung out of cold water to be applied to the chest" of the pneumonic patient. The temperature of the water, the duration, technic, frequency of repetition are left to the discretion and good or bad judgment of his hearers and readers, and yet these are far more important in hydrotherapy than are the dose, time of administration, frequency, etc., in prescribing medicinal agents.

In a paper on pneumonia, published several years ago by an eminent American teacher, the following sentence occurs: "If the temperature in pneumonia rises to 104°, 105°, or 106° F., I use cold baths or cold packs." Such indefiniteness is to be greatly depreciated when coming from one who is highly regarded as a teacher and guide. A cold bath, without stated temperature, is generally understood to be a bath to which neither hot water nor ice has been added. Such a bath would have a temperature of 75° F. in August, and 45° F. in December, in this city. Thirty degrees in the temperature of a bath are capable of producing an immense difference for good or evil; in pneumonia it would react very unfavorably, according to my personal observation. Just as Niemeyer failed to create a permanent following for the practice which he so highly lauded, so has the justly famous teacher referred to failed to impress upon the profession a practice which he had taught in 1870, as follows: "If there be anything I should rely on in pneumonia, if the temperature is very high, it is cold-water treatment." If this excellent teacher had been as explicit in giving temperature, duration, etc., of the "cold-water treatment," as he was in giving the doses of digitalis, nitro-glycerine, and camphor, the valuable lessons he endeavored to inculcate would not now (twenty-five years later) require to be taught anew; they would have served as a beacon-light to the vain searcher after reliable therapeutic methods in this fatal disease. Let me emphasize, therefore, that while I may have offered nothing novel or striking in this essay, its chief aim is to impress upon you the great import of giving definite prescriptions for all hydriatric measures in this as in other diseases, be they acute or chronic, for only by such precision may we obtain definite results.

Statistical evidence to prove the value of the measures here advocated, or of any other method of management, is indeed very difficult to produce. The type of the disease differs very much; in private practice the number of cases is too meager, and observations are too inaccurate and unreliable. I may say, however, that I have not lost a case of uncomplicated croupous pneumonia in private practice since the adoption of the management of patients suffering from it, here outlined. In hospital work the chief element of success with this method, viz., its application in the early stages of pneumonia, is unfortunately absent. Its favorable influence upon the progress of the disease is attested, however, by the statistics of 156 cases in the J. Hood Wright Memorial (formerly Manhattan General) Hospital, in which the total mortality from pneumonia has been reduced one-half, since the method here advocated has been adopted by the entire staff; while in the cases admitted before the fifth day the mortality has been 12 per cent. against 37 per cent. under the formerly prevailing expectant plan. These cases were under the observation and treatment of my colleagues, Drs. Daniels and Knickerbocker, and myself. The method described above is, with slight modifications, equally favorable in catarrhal pneumonia.

The ice-pack method recently advocated by Dr. Mays, of Philadelphia, which claims a mortality of 3.58 per cent. in private practice, is worthy of mention in this connection, because its success is applicable upon the theory that it fulfils all the therapeutic indications almost as well as the cold wet compresses. Ice is applied in bags which are wrapped in towels and placed over the affected area, with a view to reduce the inflammatory process in the lung by direct cold. That the latter is an erroneous idea has been proven by the experiments of Silex,[1] who has shown by experiments on animals, that "ice applications increase the temperature of the organs over which they were placed and which were to be cooled." This effect of local cold has long been recognized by hydrotherapists, who know that intense cold applied externally produces a compensatory hyperæmia in the parts beneath, thus protecting the latter against destruction if carried too far. The rationale of the favorable influence of the ice-compress treatment, however, is not difficult to explain upon recognized principles of hydrotherapy.

The towels which envelop the bags of ice soon become thoroughly wet by condensation, and thus the application is practically a continuous wet compress. This together with its unevenness prevents the complete reaction which is so useful in arousing the nerve-centers, but its being applied upon parts of the chest only prevents serious results from this source. The objection to ice compresses would seem to be their uneven surfaces, which render them uncomfortable when applied to the posterior portions of the lungs, the patient lying on the back; and also that their weight when applied anteriorly must render them inconvenient. In consideration of these facts I have not felt justified in applying this method of ice packs so long as good results were obtained from the wet compresses.

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[1] Müncher Medic. Wochenschrift, 1893.

In conclusion I hope to have shown that

1. Pneumonia is, like typhoid fever, an infectious disease whose chief attack is upon the nervous system, as manifested by heart failure, and other signs.

2. The treatment should be applied, not to the disease, but to the patient, whose resisting capacity should be enhanced by all useful remedial agencies.

3. Calomel in a large dose, followed by wet compresses, adapted in duration and temperature to each case, with fluid diet, and an abundance of water internally, aided, if need be, by mild alcoholic stimulation and hypodermics of strychnine, offer at the present time the most favorable, yet not perfect, results in the management of patients suffering from pneumonia.—The Medical News.

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AMBULANCE DOGS.—A writer in the Union Medicale for September 19 remarks that in the streets of the village of Lechensch, near Cologne, there may be seen a veritable battalion of dogs which have been trained for the ambulance service in view of the approaching German military manœuvers. Each animal carries on its back a small basket provided with pockets which contain all that is necessary for use in temporary dressing, also a flask of brandy. The dogs have been taught to recognize wounded persons and to lower the basket so as to enable them while waiting for the litter, to quench their thirst and to relieve their sufferings. A large red cross is marked on the basket, and a small lantern with a reflector for night service is strapped on to the animal. These dogs took part in the German military manœuvers last year, when their usefulness was demonstrated. This year their instructor has been charged with the training of another pack, for which he has chosen Scotch dogs of medium size, the intelligence and docility of which are remarkable.—Cincinnati Med. Journal.

PROCEEDINGS OF SOCIETIES.

REPORT OF RICHMOND ACADEMY OF MEDICINE AND SURGERY.

Regular Meeting, Held January 26, 1896.

Dr. J. N. Upshur, President in the chair. Dr. Mark W. Peyser, Secretary and Reporter.

Dr. J. A. Hodges read a paper on Tubercular Meningitis. [See page 199.]

DISCUSSION.

Dr. William S. Gordon said Dr. Hodges had conferred a favor upon the Academy by calling attention to the disease under discussion. He could look back to his cases and bear testimony to the fact that errors in diagnosis were easily made; and that we were frequently liable, in the early stages of tubercular meningitis, to mistake it for typhoid fever or some other affection.

Dr. Hodges had referred to the gradual onset and late convulsions as differential diagnostic points; but it was well to bear in mind that the malady was sometimes ushered in with a convulsion, the prodromata being absent or hardly noticeable. One such typical case he remembered seeing some years ago. Another point worthy of mention was the vacillating pupil in the early stages. The pupil was moderately dilated, contracted promptly but incompletely to a bright light, but returned rapidly to the previous dilatation in the presence of the light. Wide and fixed dilatation, with respiratory embarrassment, was observed in the late stages, and was due to pressure upon the bulb centers by effusion. In connection with the differential diagnosis, he would like to ask Dr. Hodges whether he believed in simple, so-called, idiopathic meningitis, irrespective of traumatism?

Dr. Hugh M. Taylor had recently treated three cases. He knew of no more appalling condition. In his own and in the experience of others, it has been attended with a mortality of 100 per cent. If a case diagnosed as tubercular meningitis got well by bromides, calomel, and the iodides, he should doubt the correctness of his diagnosis. The cases referred to were characterized by sudden invasion, rapid progress, and a clear history of infection in each. One was a strong, robust little boy, age 5 years; he was a picture of vigorous childhood. His father was in the last stages of pulmonary tuberculosis. The mother was urged not to sleep in the father's room and not to let the child do so; and was told of the contagiousness of the disease. This advice was not respected. With no premonitory symptoms, cerebral irritation, cerebral effusion, cerebral pressure, stupor, convulsions, coma, and death quickly ensued. From vigorous health to death occupied only eight days.

A second case occurred in a child, a victim of quiescent morbus coxarius. Four or five days before his death this child was on the street as well as he had been for years. Cerebral irritation, quickly followed by cerebral effusion, stupor, coma, convulsions, and death ensued.

The third case was that of a child, age 3 years, with active spondylitis in the upper dorsal region. Pachymeningitis of the spinal dura ushered in an infection with rapid extension to the brain envelopes, with effusion, pressure effects, stupor, coma, and death in a few days.

As he watched these cases with a full knowledge that there was but one possible outcome, *i. e.*, speedy death, he could but agree with others that some other plan of treatment than that usually followed was justifiable. A mortality of 100 per cent. could not be made worse, and force the conviction that such a desperate condition warrants heroic remedial resources. Diffused or circumscribed septic pleuritis, pericarditis, and peritonitis and affected joints, called for operative interference. In the brain apart from the septic processes, intra-cranial pressure incident thereto must be quickly removed. Mixed infection, he thought, must play an important role in the intracranial sepsis, the tubercular deposits and focus acting as factors which diminish tissue resistance and favor mixed infection. Removal of the effects of intra-cranial infection and its consequences—brain disorganization and perturbed function—could only be accomplished by operative interference. We do not hesitate to drain other serous cavities, and irrigate them, and we should not, in the face of such dire necessity, fail to do something, hesitate to open, drain and irrigate when we have the ventricles distended with septic fluid in meningeal inflammation. In 1873, Dr. S. W. Gross urged the value of puncture, irrigation, and drainage, and his tabulated reports at that time showed 45 per cent. of recovery. If such marked success could be obtained twenty-five years ago, with improved technique and asepsis we should hope for a decided lessening of the death by operative interference. Etiologically, he thought we should regard the intra-cranial pressure occurring in case of tubercular meningitis as mixed infection; and, clinically, should be placed in meningitis, with streptococcus infection from any other cause. Tubercular peritonitis had come within the pale of rational surgery, and it was along the lines of ventricle or spinal puncture and drainage and irrigation that we must look for advance in the treatment of tubercular meningitis and its sequences, effusion, intra-cranial pressure, stupor, coma, convulsions, and death.

Dr. Gordon, referring to Dr. Taylor's remarks, said it had occurred to him during the discussion that temporary abatement of the symptoms might be obtained by tapping and aspirating the spinal membranes in the lumbar region, but he did not know that the procedure had been conceived or accomplished. Whether this operation would do for the disease what drainage of the abdomen had done for tubercular peritonitis was doubtful, but any measure was justifiable which held out the faintest prospect for relief. Of course, the ideal operation by analogy would be to open the ventricles.

Dr. Jacob Michaux reported two cases, both ending in recovery.

Case I.—Aged 18 months, resulting from or following measles. First, there were dysenteric symptoms, then meningeal, *e. g.*, retraction of the head, distortion of the eyes, convulsions, etc. Calomel was freely administered, and the head blistered.

Case II.—Aged 12 months; had symptoms practically the same, but it was primary. Treatment was the same. He did not use opiates, as he feared the tendency of increased circulation in the brain.

Dr. R. D. Garcin reported a case similar to the first of Dr. Michaux's.

Dr. Paulus A. Irving differed from Dr. Michaux in diagnosis. Unless there was a previous history of tuberculosis it could not be called that form of meningitis. It was rather spurious hydrocephalus.

Dr. Landon B. Edwards said Drs. Michaux's and Garcin's cases were streptococcic forms of cerebro-spinal meningitis. There were four cardinal points to be remembered—headache and vomiting, fever and delirium. It was almost impossible to get a succinct history of the case. Tubercular meningitis rarely begins before the second year, and it was not in its nature to recover. At the same time, there was a development of tuberculosis in some other part of the body. A diagnostic point was the fever, which was not as high in this as in other forms of meningitis.

RICHMOND ACADEMY OF MEDICINE AND SURGERY.

Regular meeting held February 9, 1897.

Dr. J. N. Upshur, president, in the chair; Dr. Mark W. Peyser, secretary and reporter.

Dr. Ramon D. Garcin read a paper on "Lobar Pneumonia." [See page 203.]

DISCUSSION.

Dr. Landon B. Edwards said Dr. Garcin was correct in regard to phenacetine, if we undertook to relieve pain and bring down fever, when used with caution and in moderation. Than strychnine, no agent is more valuable; and in combination with digitalis, it aids resolution and sustains the heart. He did not think enough stress had been laid upon the use of jackets of cotton batting or oil silk in this condition.

Dr. Moses D. Hoge, Jr., referred to the application of blisters, saying that by some it was used as a derivative, which was nonsense. It was not proper to apply them in the early stage, the only benefit here being the relief of pain; the disease cannot be limited. The best time for their employment is during the stage of resolution. Up to this time, there is little blood in the capillaries; but when resolution sets in, the blood-vessels dilate, some pus is expectorated, and some is got rid of by the lymphatics. The counter-irritant and stimulating effects are best obtained in this fourth stage. Recently, the inhalation of chloroform and alcohol in equal parts has been advised to abort pneumonia.

Dr. J. W. Henson remarked that he had a fixed rule regarding the use of the blister. He puts it on as soon as he discovers pneumonia, and its benefit is a purely reflex vaso-motor one. Why apply blisters for acute inflammations at other points? He has never seen a case hurt by it, and not one not benefited. The following case was cited: Man, aged 30 years, sick nearly 24 hours. There was violent pain in the head, and over the lower lobe of the right lung; temperature, 104° F.; respiration, rapid; pulse, full and bounding, and consolidation almost complete. A blister 5x7 was applied. On the following morning there was no temperature, no pain; the man was hungry and desired to return to his work. There were no signs of disease from that time on. The only cases seen not benefited by blisters were inebriates and the old.

Dr. C. R. Robbins spoke of the use of hot bricks under the bedclothes to produce copious sweating, being renewed from time to time. They were applied from the beginning, and hastened the progress of the disease, being analogous in their action to cotton batting. One drug he used to almost the exclusion of others was the aromatic spirits of ammonia.

Dr. W. S. Beazley agreed fully with Dr. Henson regarding the use of the blister. He applied it in every case at once, and had yet to see a bad result.

Dr. Virginius W. Harrison had never used a blister; had never seen occasion to do so. He applied the cotton jacket and stimulated the patient.

Dr. Edwards said Dr. Henson's case was probably a lobular pneumonia or a bronchitis that might have developed into lobar pneumonia if let alone. The blister might not do damage; but would it do good? He did not see the necessity for it; it was too violent. If at the beginning the individual was healthy, he would get well, unless a complication ensued. He did not believe blisters had a particle of benefit in the first stage of lobar pneumonia.

The President remarked that he was more and more convinced of the gravity of lobar pneumonia, and more and more at sea as to its management. But he was convinced of the value of certain remedies. For one thing, he was at sea concerning the definition—an acute, infectious, self-limited disease—but he has over and over again cut it short in the first stage. He had been told he had cured congestion. What is the inflammation but congestion? Recently he saw a case in which there were all indications of collapse. He was called to it at 4 A. M. The patient was in such agony that nothing could be determined, but, probably, friction sound. He gave what he much deprecated—morphine—and the patient slept four hours, awaking with pain, which disappeared upon the application of a blister. She was benefited by what is described as counter-irritation. He could recall but one case which came to a crisis, and was self-limited. He believed that drugs might add to the comfort and safety of the patient. He did not believe all cases were caused by germs, which developed because the soil is fertile, rather than vice versa. Osler cannot explain pleurisy by a germ.

Treatment.—Quinine, phenacetine, full doses of carbonate of ammonium. He had seen the last bring patients back almost from the verge of the grave. It did good not only by stimulation, but by its property of promoting the fluidity of the plastic material in the blood. Diaphoretics should be given in the early stage to lower the temperature and get the blood into the skin. He believed blisters of benefit, but did not always apply them in the first stage. Morphine, in full doses, was contraindicated. To one agent he called attention, Fowler's solution, which was especially valuable in the stage of resolution. Valuable in the early stages were full, free applications of turpentine stupes; and at the commencement, cotton batting jackets and flaxseed meal poultices.

Case I. refers to that already mentioned. It was on a Tuesday he was first called. Both lungs were intensely congested. He regretted having given morphine and not blistering immediately. The case continued in gravity without responding to treatment. Wednesday night was spent at the bedside of the patient, and at 7 A. M. he left the patient cyanotic, etc. At 10 A. M. she died.

Case II. was a sister of Case I., aged 53 years. At midday Tuesday she complained of feeling unwell. By midday Wednesday she passed into a comatose condition, and was dead at 5 P. M.

Dr. Hugh M. Taylor believed that the cases reported by the President were proof positive in themselves of infection.

The Secretary, referring to the oxygen treatment as mentioned by Dr. Garcin, said that while he had had no experience with it in pneumonia, he believed he had used it with entire success in a case of capillary bronchitis. His method was the vaporization of hydrozone by the apparatus made for the purpose. He believed it would be valuable in pneumonia. Commenting on cold-water applications, he said the beneficial results, in his opinion, were not alone direct as by the reduction of fever, but also due to the production of a leucocytosis.

Dr. Henson said he would not be convinced that pneumonia was not benefited, and perhaps cut short, by the application of flying blisters, until reported cases had been diagnosed by the microscope in the early stage, and those cases subjected to the treatment by blisters without benefit. It was a matter worth thorough investigation in that line, for it was the only way to decide positively whether or not blisters would aid in true lobar pneumonia. If blisters did no good, we ought not to use them; and conversely, if they benefited, we by all means should apply them.

In regard to Dr. Edwards' criticism of the diagnosis of the case mentioned, it was rather peculiar to have a bronchitis limited to one lobe and producing complete consolidation of that lobe, with absolutely no trouble in other parts of the lung; and all this with a temperature of 104°, the patient being a young and very robust adult.

Dr. Henson said he had seen a blister reduce temperature from 104 to 101° when the patient had profuse expectoration.

Dr. H. H. Levy remarked that his idea was that pneumonia was a self-limited disease, and that he had treated a number with critical terminations, without blisters and almost no medication. He had seen them terminate as early as the seventh day, and sometimes earlier, sometimes later. Ordinary cases may get well without any nursing, while others require much care. If pleurisy were conjoined, then a blister might be of benefit. If resolution were retarded, he might be inclined to put on a blister, but this was theoretical after all. In the old, he employed stimulation by means of ammonia and whisky.

REPORT OF EIGHT CASES OF PARAPLEGIA CURED.

Dr. J. Allison Hodges reported eight cases of paraplegia cured during the last year. Three of them were cases of paraplegia from multiple neuritis, in which the paraplegia had been well marked for six, thirteen and eighteen months in patients aged 9, 17 and 19 years respectively. The treatment in these cases was substantially the same, namely: Salol, quinine, swathing the limbs, faradic electricity, warm alkaline baths and hygienic regimen. Another case was traumatic myelitis in a young girl, aged 21, who had been paralyzed for a little more than twelve months, by a fall on roller skates, the injured vertebræ being those of the lumbar region.

Modified rest and suspension, together with forced feeding, were the only therapeutic measures used. A fifth case was, after a month's treatment, diagnosed to be a hysterical paraplegia, and treatment was directed to that end with most gratifying results.

The sixth case was one of those rare cases of paralytic astasia abasia that are so nearly allied to hysteria. The seventh and eighth were cases of syphilitic myelitis one of them occurring during the regular succession of specific symptoms; the other not supervening till after the cerebral syphilis, and not yielding to treatment until immense doses of the iodides had been given.

TRI-STATE MEDICAL SOCIETY OF ALABAMA, GEORGIA AND TENNESSEE.

Eighth Annual Session held in Chattanooga, Tenn.

The following is an abstract of the papers and discussions:

Y. L. Abernathy, Hill City, Tenn., presented the first paper, "Convulsions in Children, Treated with Large Doses of Morphine," in which he related several cases about to die, but which recovered under large doses of morphine.

Andrew Boyd has used large doses in cholera infantum. Considered it rational in convulsions no matter how large the doses might seem.

J. P. Colvin said that they could be controlled by cold applications. Did not like to advocate such large doses as morphine if long continued would produce convulsions.

P. L. Brouillette thought that these cases would tolerate large doses which relieves symptoms and gives time to find and treat cause.

W. C. Townes agreed in giving large doses of dangerous medicines in exceptional cases. Had given ½ gr. morphine to children. Had given gr. j. strychnia with good effect. [Possibly a fatal effect was intended.—Editor.]

George S. Brown thought it dangerous to allow claims of harmlessness of such large doses to go unchallenged. Convulsions might be caused by morbid material in alimentary canal which might be swept away as with a cathartic when the morphine would likely prove fatal.

W. G. Bogart said that the author of course gave other treatment. Besides a cathartic an enema should be given.

Dr. Abernathy closed the discussion by saying "Morphine is one of the most damnable, detestable, deadly remedies we have, like the vampire sucking the life blood of its victim after lulling him to sleep. I never use it in children except in cases which otherwise will surely die.

"The convulsion is only a symptom, but a very dangerous symptom, and if not relieved patient will die."

D. S. Middleton, Rising Fawn, Ga., read a paper, "Cystitis: Report of Cases," being a statistical report of cases showing its frequency in women, a discussion of its pathology, cause (microbic infection), the treatment by antiseptic injections, and a report of cases so treated.

G. A. Baxter would add only one remedy, a saturated solution of acetate of aluminum. Nitrate of silver sometimes produces tenesmus, and he no longer uses it.

George S. Brown commended the paper for comprehending the main points without wasting time, the regard for authorities while still having opinions distinct from them based on close and scientific observation. He called attention to the practice of introducing a thin rubber bag, smeared with 20 gr. ointment, ichthyol ointment in gelatine. The bag is inflated and the ointment comes in contact with the mucous lining.

Y. L. Abernathy mentioned as a cause in the male stricture of the urethra. If these are cured the cystitis will disappear.

R. R. Kime, Atlanta, Ga., read a paper, "Some Obstetrical Complications with Report of Cases."

1. Unicervical Septus Uterus with abortion from right side. Fœtal structure had passed, decidual structures retained, and infected removed, then right side disinfected and drained. Right side 3½ inches; left (non-gravid) 1¾.

2. Uterine hæmorrhage, eight days after labor due to typhoid fever. Had to be tamponed three weeks, changing one to three days. Changes rapidly made to prevent bleeding to death.

3. Placenta Previa Centralis. Infection before delivery. Had bled four or five days when first seen. Immediate delivery by carrying hand into uterus and performing podalic version, peeled off placenta, irrigated uterus, and dressed antiseptically. Symptoms of infection returning with renewed vigor and not being controlled by morphia and phenacetine, administered salines, irrigated, disinfected and drained with relief in a few hours. Drainage kept up six days, when pulse and temperature were normal.

4. Tumor and infection complicating labor. Treated by irrigation and tubular drainage. No evidence of tumor one year later.

5. Extreme septic intoxication seen ten days after labor, established utility of tubular drainage beyond doubt, as when removed constitutional symptoms returned three times. Gauze failed to meet indications. Gauze and curette in septic infection condemned.

W. G. Bogart indorsed the position of the writer, especially the packing with gauze to control hæmorrhage, but would also flush the cavity with hot antiseptic fluid. He also commended the treatment of fever with antiseptics rather than antipyretics, followed by free drainage, and tubular drainage is the best. Cleanliness, quinine, alcoholic stimulants, avoiding antipyretics, especially coal-tar derivitives is the rational treatment of these cases.

In closing, Dr. Kime said that very hot water was used to check hæmorrhage, also solutions of alum, of boric acid, of iodine, but all failed in case 2. Ordinarily hot solutions of iodine will check bleeding. Gauze tampons do not brain. The curette should also be condemned as it breaks down nature's barriers and opens avenues for absorption. The ideal is to carry a strip of gauze in with drainage tube giving tubular and capillary drainage.

W. C. Bilbro, Murfreesboro, Tenn., reported, "A Case of Bradycardia" from ptomaine poisoning. The history was one of indigestion. Pulse was 30 standing, after walking two blocks. At times pulse was 18 and again 6, but regular. Improvement on strychnia, arsenic, etc. There are two classes, physiological, as in the puerperal state, and pathological, as from exhaustion in continued fevers and in stomach affections.

J. R. Rathmell wanted to add to the above neurotic cases. Physiological is probably a misnomer, as diseased conditions have been found post-mortem. Symptomic bradycardia is quite common.

R. R. Kime related a case where the pulse was 36. Listening to the heart sounds the beats were 72, every other beat only being felt at the wrist..

Dr. Bilbro said that he had called attention to the importance of counting the apex beat rather than the pulse at the wrist.

J. R. Rathmell, Chattanooga, Tenn., read a paper on, "Scarlatina," and laid stress on the complications which are the cause of death, and to which treatment should be directed. The contagion resides in the epidermal scales thrown off during desquamation, hence the necessity of isolation, especially during desquamation, also the need of a vigorous effort to destroy the almost imperishable germs hidden in the room, by fumigation, by boiling, and even by burning all articles of clothing, bedding, etc., used during the attack, and a thorough renovation of walls and woodwork with kalsomining and paint, in order to prevent a possible attack in future.

Y. L. Abernathy dwelt on the vitality of the contagion and its latency, and related cases illustrating these points.

G. W. Drake said the diagnosis of a typical case was of more importance to the public than the treatment. On the mountain he had encountered a case where there had been none in the history of the place, and the contagion could not be accounted for. There were three other cases none of which could be traced to the first. The origin could not be accounted for.

G. W. Mills gave an account of an epidemic in which the origin of the first case could not be traced.

Dr. Rathmell, in closing, said that there was such a relation between this disease and diphtheria that when we had one we were likely to have the other. He did not believe them to be the same.

G. A. Baxter, Chattanooga, Tenn., described "A New Splint for Fractures of the Humerus Below the Surgical Neck," and demonstrated the same on a subject. It consists of a blunt wedge of tin for the axillary space to which is attached a right-angle splint for the arm which is adjustable on the humeral portion, allowing extension and fixation before bandaging. The axilla is made the point of counter extension, extension from elbow. It allows examination in compound fractures without disturbing dressings.

D. S. Middleton related a case treated with plaster paris applied while swollen, when this disappeared there was overlapping. This splint seemed more satisfactory.

In closing, Dr. Baxter said that statistics showed as much as 33 per cent. were ununited and this splint was invented to meet the indications as found in his own cases. Fixation cannot be from shoulder, it must be from a fixed point. There will be no complaint from numbness of fingers or pressure in axilla. Any tinner can make splint.

C. R. Achison, Nashville, Tenn., read a paper, "Treatment of Cancer of the Skin," advocating the use of caustics in the treatment of epithelioma. There is much less destruction of tissue than with the knife. In the use of the latter, if there are any cells left, there will be a recurrence, while if caustics are used the inflammation, etc., will cause the death of the pathologic cells beyond the point cauterized. The pain can be reduced to a minimum by mixing with cocaine or general anesthesia. Arsenious acid has a selective action devitalizing the cancer cells. Caustic potash is specially useful on the lip. Zinc chloride produces a dry slough.

P. L. Brouillette asked if any of the members had ever arsenical poisoning from the caustic. In one case he had applied it to too much surface and had poisoning. The pain was relieved with morphia. The final result was happy. In epithelioma of the nose he used the electrocautery, which answered every purpose.

G. A. Baxter said that the argument of the surgeon was that by the knife the patient escaped the pain which in some cases was excruciating. He used the paste in proper cases, but believed the knife generally the best, shortens the time, gives physiological regeneration instead of suppurative results, and is equally efficient in eliminating the diseased products.

Frank Trester Smith called attention to a paper read before the American Medical Association, and found in the JOURNAL of October 3 in which he had advocated the use of caustics for epithelioma of the lids, and related a case.

W. C. Bilbro thought the knife should be suggested first to the patient with deep-seated cancer, and if refused then the caustic.

J. B. Murfree thought in Dr. Brouillette's case poisoning was due to too weak a paste. There should be two parts of acid to one of gum, or it might be stronger so as to destroy the tissue and not irritate.

Dr. Brouillette said that he had used one part of gum arabic to two of acid.

Dr. Achison said that he had looked the matter up and had never been able to find a case of poisoning. In cancer of the breast it was not scientific to use caustic, but if the patient refused the knife we ought to use the caustic and not allow the patient to go to some quack.

W. Frank Glenn, Nashville, Tenn., read a paper, "Diseases of the Veru Montanum (Caput Galliganis)." He described acute and chronic inflammation, hypertrophy, hyperesthesia; cause gonorrhœa and masturbation, hyperesthesia especially due to masturbation. Symptoms: Frequent desire to urinate with dribbling at end of act. Dribbling urine. Chronic inflammation most frequent cause of impotence. Internally he recommended alkilithia or maizo-lithium. Locally argonin. Nitrate of silver should be avoided in acute cases. Recovery is slow. General tonics, galvanism, etc., will generally cure hypertrophies. In hyperesthesia there is premature ejaculation in the sexual act. The mistake here is that the case is often treated for sexual weakness with strychnia, phosphorus, etc., while the opposite course is indicated.

C. R. Achison believed the cause of the difficulty in treating genito-urinary diseases lay in the veru montanum. In hyperesthesia seminal emissions the stimulating treatment is contra-indicated, and the doctor has given the classical treatment, the cold current and sexual sedation. G. W. Drake asked how the disease caused premature ejaculation. Dr. Glenn closing, said in the same manner as the lids close when there is a foreign body in the eye. His former treatment caused these cases to get worse, but the present treatment was successful (sedation, cold water). Argonin 5 per cent. was preferable to silver nitrate, as it was painless.

George S. Brown, Birmingham, Ala., read a paper entitled "The Bacteriological Data of Peritoneal Drainage." The emphasis lay on the bad principle of introducing drains after abdominal operations, on account of a fear of infection, say from an outpouring of pus from ruptured tube. The peritoneum has a great destructive power of its own, both to the germs and to their toxins when its integrity is not harmed. A drain is a good thing where there are infected surfaces already, or where the peritoneal surface has been greatly damaged in the presence of an infectious agent, such as ruptured tubes. But a drain harbors germs, and all wounds will suppurate in which drains are used, while few or none would if drains were not used. W. D. Haggard. Jr., said the experience of practical surgeons was in favor of drainage. The natural drainage is through the vagina.

G. R. West said the paper gave a scientific explanation of his own experience.

Prof. S. H. Dodson, of the Chattanooga Normal University, read a paper, "Physiology of the Senses." He said that the old school of mental philosophers studied deductively the new school inductively. Sensations the first form of mental life, hence the importance of their study. Sensations differ in quality, have their local signs and can be definitely located on the surface of body. They are transmitted to the brain in two ways—by periphery of cord and through the gray matter of the cord. This last is the after-image of touch. Sensation of effort is peripheral sensation of movement located in the head. Sensations of taste and smell often confounded. He related a number of experiments illustrating this.

J. P. Stewart related a case where patient had lost sense of smell. By these experiments most of our knowledge comes.

G. W. Drake called attention to the proximity of the centers of taste and smell. They probably overlap.

Professor Dodson said the imagination was of great importance. He related a case where a man was shot with blank cartridges, but who died at the suggestion of the surgeon.

W. D. Haggard, Jr., Nashville, Tenn., read a paper on "Vaginal Hysterectomy for Bilateral Suppurative Processes of the Uterine Adnexa." He gave the following reasons for removing the uterus: 1st. A large number of cases where tubes and ovaries were removed were not perfectly cured, pain being persistent, hysterectomy cured these. There were painful malpositions, a more stormy and protracted menopause. There was danger of adhesions to the hollow viscera and subsequent obstruction. It takes no longer to do a total hysterectomy than curetting or ventrofixation after double ovaritomy; the mortality is lower. The vaginal method was preferable because the preliminary step in vaginal section allows thorough exploration and conservative treatment with minimum of risk. 2d. The vagina is natural approach and logical avenue for drainage for pelvis. 3d. It is immune from unpleasant sequelæ of laparotomy; possibility of hernia, stitch abscess, infected ligature and abdominal supporter. 4th. Less immediate shock. Convalescence is smoother and shorter. 5th. No exposure or handling of intestines. 6th. Less danger of peritoneal contamination. 7th. Mortality is lower. 8th. Invades only diseased area and leaves undisturbed protecting mass of adhesions.

W. E. B. Davis, Birmingham, Ala., read a paper on "The Treatment of Pus in the Pelvis." He said the French surgeons reported inability to remove the appendages in some cases of vaginal hysterectomy for pus in pelvis, but that patients recovered, demonstrating that drainage would cure many cases.

Vaginal incision for pus in pelvis not confined to tubes had been practised a long time with good results. Many such cases require no further surgery. He claimed that large abscesses could be drained through the vagina with permanent recovery in a large proportion of cases where vaginal hysterectomy is recommended so highly by the French surgeons. If not relieved the condition would be better, and later an abdominal operation could be done and diseased appendage removed. It is very exceptional that uterus will have to be extirpated.

J. A. Goggans discussed these two papers, saying that he followed the practice of Dr. Davis. We should consider harmful sequelæ of complete ablation of genital organs in young women. Every appropriate treatment was justifiable in view of the variety of pathological conditions.

Dr. Haggard said that conservative methods should be exhausted. In chronic cases the uterus is diseased and will cause untold misery.

Dr. Davis said that Dr. Haggard was sustained by many eminent men in his position. When these organs are removed there is a condition of the nervous system which causes a little pain to be exaggerated into excruciating suffering. A large proportion of these cases can be cured without removing the uterus, which is an important organ after removal of ovaries and tubes. A woman is thus more natural and the vagina does not shrivel up.

Willis F. Westmoreland, Atlanta, Ga., read a paper, "Some Remarks on Syphilis," saying that it was often communicated by means other than sexual intercourse: 60 cases infected from a catheter, a dozen children from a vaccine point. Hereditary syphilis is quite common.

G. A. Baxter took the ground that it would be proper to prevent the marriage of a syphilitic even if it might seem to violate professional secrecy, through a threat of exposure to the patient.

R. P. Johnson condemned the custom of indiscriminate kissing of babies, which was an outrage on a being powerless to defend himself.

Y. L. Abernathy had never seen a case contracted in the manner described in thirty years' practice, but did not doubt their existence. From neglect there are more deaths from gonorrhœa than from syphilis, which is more amenable to treatment.

C. R. Achison said that the virus was bland and had to come in contact with an abraded surface. This tends to protect the innocent. Cases of such contracture are few and far between. He did not see that we could do anything about it.

Dr. Westmoreland thought that we could do much about it, as in servants with lesions by compelling them to quit work, also by preventing the marriage of an innocent girl to a syphilitic.

J. B. Murfree, Murfreesboro, Tenn., delivered the president's annual address, "The Doctor of Medicine." He congratulated the society on its prosperity. He advocated high and unselfish motives in the practice of medicine; deprecated men entering the profession simply to make money. Medicine is a poor trade for an honest man, and if money is the only object there are others that pay better. The doctor's first duty is to his patient, to give him the best services. He should be ethical, not allowing the love of gain or distinction to swerve him from the right. The reputation of a brother physician should be defended. He dwelt on our duty to the public in regard to preventative medicine, and how to avoid quacks and patent medicines.

J. A. Goggans, Alexander City, Ala., reported "A Few Unique Cases in Abdominal Surgery." He presented a specimen of ovaries that contained serous and myomatous multiple follicular cysts, from a woman fifty-five years of age. Related a case of abdominal section for tubercular peritonitis. The patient had been an invalid seven months. Over half a gallon ascitic fluid evacuated; abdominal cavity irrigated with salt solution. Recovery was good and patient able to work most of the time. He presented the stomach and intestines of the third case. Diagnosis was impossible without exploratory incision. Fourth case was that of right hemiplegia following an exploratory incision in a woman sixty years old. She was recovering from the paralysis. The fifth was the only case that has ever recovered after operation in America, and perhaps the only case where operation has been undertaken for exactly the same condition in this country. The patient had been out of health for two years, but her abdominal pains and abdominal enlargement had existed eight or ten months. She had been treated for abdominal dropsy. On opening the abdomen the tumor proved to be one of the mesentary. He stitched and drained, and the patient recovered. The other successful cases were one operated on by Bantock in London and one by Pean in Paris.

W. E. B. Davis had seen Dr. Goggan's case of mesenteric cyst in consultation, and regarded it as one of much interest.

Katherine R. Collins, Atlanta, Ga., read a paper entitled "Microscopical and Chemical Aids to Diagnosis," dwelling on the importance of examinations of the urine of which the whole amount should be examined five days in succession. The morning urine is that of a fasting patient. She related cases and called attention to the use of the microscope in diagnosing many diseases, especially tuberculosis, diphtheria and malaria, and the changing views in regard to the origin of typhoid fever. The Eberth germ according to Vaughan not being the cause, but an involution form of one of several germs that may separately or collectively be the cause of the disease.

George S. Brown stated that to the microscope alone is due the credit of almost all that is accurate in our art, all that dignifies medicine as a science. The "bug" theory rules everything, and even the belated ones who sneer at it cannot deny its general acceptance. Nothing can compare with its clinical use in examining the blood in malaria, inflammation, tuberculosis, measles, scarlet fever and all anæmias which have been heretofore a jumble of guess work. Examination of sputum absolutely the only way to diagnose tuberculosis.

R. H. Hayes, Union Springs, Ala., read "A Statistical Report of Some of the More Recent Remedies Used in the Treatment of Tuberculosis and Summary of Recent Preventative Methods of Value," being an array of statistics from eminent men and a report from a committee of the New Orleans Medical Society in favor of the newer or biological remedies such as tuberculin, antiphthisin, nuclein preparations, and such medicines as pilocarpine and aseptolin, which were good evidence in favor of these remedies as they were clinical, and were from men prepared to apply them properly, carefully, and who had taken the proper time. He also gave an outline of the work of the New York Board of Health to restrict tuberculosis, being public and private instruction as to danger from milk and meat (bovine tuberculosis) and from dried sputum (human tuberculosis), it having been conceded the majority of cases were contracted. The Board had gotten better results in preventing spread of disease and had largely reduced mortality rate. It had demonstrated beyond doubt that tuberculosis was contagious, infectious and preventable.

Y. L. Abernathy thought these remedies were made to sell. Koch's lies, d——n lies and statistics. Something may develop along this line as good as vaccination, but it is still in the future. At the present rate we will ere long have

An extract of muscle for rheumatic pains,
A gray matter extract to nourish our brains;
An extract of teeth for the fellow who can't chaw,
A maxillary extract to cure lock-jaw.
An extract of semen to cure old men,
An extract of clitoris to raise a number ten;
An extract of Hymen to preserve a maidenhead,
An essence of vagina with sea shell tints of red,
For the benefit of old bachelors grim, and old and gray,
Who can't and won't get married, coz they ain't built that way.

George S. Brown said that Koch had done more than any other man to reduce the mortality from tuberculosis not from serum therapy, but because the disease was better understood. The principle that will cure consumption has not been discovered, while it has for diphtheria, and antitoxin is a success.

Louise Eleanor Smith, Chattanooga, Tenn., read a paper on, "The Turkish Bath: Its Therapeutic Indications," giving as indication torpid liver, rheumatism, sleeplessness, malaria, typhoid fever, gout, kidney trouble, colds, etc.

Katharine R. Collins doubted its utility in weak circulation while it was indicated in engorgements. There is little literature on the subject.

R. R. Kime said that difficulty was in preparing the apparatus, especially in private houses; freer use of water would be of benefit.

Y. L. Abernathy said there could be no doubt of the value of hot water in syphilis and rheumatism, and other blood diseases. People spend hundreds of dollars at Hot Springs to bathe in hot water, for there is no virtue in the water. Even if it were medicated none of the medicine would be absorbed.

K. H. Davis endorsed the use of water internally, externally and eternally.

Dr. Smith closed, by saying that the heart gets stronger under the bath. One could be improvised with a lamp and a blanket. The cold spray could be easily applied.

J. P. Stewart, Attala, Ala., read a paper, "Medicine: Hippocratic and Operatic," giving a review of the history of medicine, commenting on homœopathy, eclecticism, etc., closing with the Hippocratic oath.

G. A. Baxter objected to classing Koch and Pasteur with the operatics. They were not responsible for improper use of their remedies.

H. Berlin said we were not sure Hippocrates was the man he was represented. He was charged with setting fire to the temple of Corinth. Human nature was the same then as now.

George S. Brown said that quackery was the remnant of what regular medicine was in the past. He cited Paracelsus who advised the young doctor to find out as much as possible from the messenger so as to impress the patient and his friends with a ready diagnosis and other equally quackish methods which show that medicine was largely pretense. Quackery cannot stand against scientific medicine. Our most effective weapon is to keep abreast of modern research.

Dr. Stewart disclaimed against any intention to reflect an Pasteur and Koch; as was shown in his paper, charlatans used such names to bolster up their cause. That Hippocrates was the man he is held up to be is shown by the esteem in which he was held by his contemporaries, Plato and others.

P. L. Brouillette, Huntsville, Ala., read a paper on "The Therapy of Antipyretics," protesting against the indiscriminate use of antipyretics in fever.

Fever is nature's remedy for certain conditions. Nature's antipyretic is cold water. Should be used for simple reduction of temperature. Quinine in large doses is a valuable antipyretic, also antiperiodic.

J. P. Colvin considered water the antipyretic par excellence. Mode of use governed by condition of patient. Full bath rarely necessary for infant. Quinine next best antipyretic, especially in sepsis. Would caution against continued use of coal-tar derivitives, especially if weak heart, lung complications or sepsis. Cold water can be used for unlimited time without impairing vitality of patient.

J. R. Rathmell used the cold sponge bath. The profession has gone to extremes with some of these new remedies. Now about the happy medium, and the coal-tar derivitives are used scientifically. In connection with these he uses the cold sponge bath.

H. Berlin said that the antipyretics were the greatest gain in antipyretics, which were not more harmful than many other remedies. A temperature of 105 would do little harm for a short time, but if long continued would be dangerous.

G. R. West said we should treat the cause. The indications for the coal-tar derivitives were analgesics rather than as antipyretics.

W. F. Westmoreland said the continued use of antipyretics would depress the vital forces. They should be given so as to have their effect at the height of the pyrexia. Sometimes the shock of cold water prohibits its use; here tepid water should be employed. Cold would do more harm than good.

R. R. Kime said the cause should be removed if possible, then consider if antipyretics would depress more than fever. Would condemn use of the coal-tar derivitives in septic infection.

P. L. Brouillette regarding cold water as best antipyretic, next to that quinine. Patient should be put in bath and cold water added.

Seale Harris, Union Springs, Ala., read a paper on "The Treatment of Puerperal Eclampsia," in which he claimed that veratrum viride was the remedy par excellence, as by lowering arterial tension it acts not only by its effect on the arteries, but by a direct effect on the heart and pneumogastric nerves, lessening the heart's action and the amount of irritating toxins flowing to the nerve centers. It has also a sedative effect. He uses 15 to 20 drops hypodermically with 1-6 to 1-4 gr. morphine, and continues 5 to 10 drops often enough to keep pulse at 60. When so used it will control the convulsions, giving time to restore the functions of kidneys and bowels, or to remove the offending fœtus.

R. R. Kime said the veratrum was our safest and best remedy. Is said to act by bleeding patient in his own vessels, by sedation and by arousing glandular secretion. Bleeding indicated in the plethoric, but this favors infection.

W. F. Westmoreland thought the explanation that the veratrum acted by bleeding in her own vessels not sufficient. There is some action we do not understand.

J. P. Stewart related three peculiar types. No. 1 patient was two months in expectancy. Chloral, morphine, chloroform and blood-letting gave no relief, so abortion was produced; recovery. No 2. Convulsions occurred in normal labor; veratrum, morphine, chloroform failed. Immediate instrumental delivery gave instant relief. No. 3 occurred after parturition controlled by morphine and chloroform.

B. S. Wert used veratrum. Cases should be delivered at once.

G. R. West believed in veratrum, but had no experience. Treated his case successfully with morphine hypodermically, chloral by enemata and chloroform. Advised emptying the uterus. Where blood-letting seemed to be indicated considered it best to allow the blood lost from uterus to be sufficient.

The following were read by title: "The Woodbridge Treatment of Typhoid Fever," J. W. Duncan, Atlanta, Ga.; "Diseases and Treatment of the Accessory Sinuses of the Nose," B. F. Travis, Chattanooga, Tenn.

The following officers were elected for the ensuing year: President, Willis F. Westmoreland, Atlanta, Ga.; vice-presidents, M. B. Hutchins, Atlanta, Ga.· Seale Harris, Union Springs, Ala.; C. R. Achison, Nashville, Tenn.; secretary, Frank Trester Smith, Chattanooga, Tenn.; treasurer, Geo. R. West, Chattanooga, Tenn.; chairman committee of arrangements, W. D. Haggard, Jr., Nashville, Tenn.

Adjourned to meet in Nashville on the second Tuesday in October, 1897.

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ABSTRACTS.

PATHOGNOMONIC SIGNS OF CONGENITAL SYPHILIS.

P. Silex (Berliner clinish Wochenschr., 1896, xxxiii, 139). The following is taken from an address delivered before the Berlin Med. Gesellschaft:

The author recognizes three characteristic signs of congenital syphilis. The first relates to the eyes, the second to the teeth, and the third to the skin. As the only real pathognomonic symptom relating to the eyes, he mentions chorioidea areolaris, in which are found, scattered over the fundus particularly in the neighborhood of the macula, black points and patches, which present here and there white spots of different size and larger areas with a black border. These represent atrophic colonies in the chorioidea, and pigment patches derived from the pigment of the stroma and pigment epithelium. The retina also being involved vision in these cases is always very much impaired. Mercurial inunctions and exhibition of potassium iodide effected no change. In a few cases, the process, which is rare, remained unilateral. Of the numerous deformities of the teeth usually mentioned, S. only considers that one form pathognomonic, where the permanent upper incisors present a central excavation, denuded of enamel, beginning on the surface for mastication, and continuing upward in the shape of a crescent. As a sign which is only found in congenital syphilis, he considers the well-known scars radiating outward in straight lines, which do not confine themselves to the corners of the mouth or to the lips, but radiate further to cheek and chin. The histological examination of a case which was particularly marked, proved that these lines are not scars in the anatomical sense, as papillæ, glands, and vessels were well preserved in the tissue under consideration. Very likely the peculiar furrow-like appearances, which are called pseudo-scars by S., are due to a muscular tension of the skin. These three kinds of conditions which were demonstrated by the author both on the subject and through illustrations, are considered by him absolutely pathognomonic. So that the presence of even one of them will lead to a positive diagnosis of congenital syphilis.—Pediatrics.

OXYGEN AS AN ANAESTHETIC.

In The Medical Record, September 12, Dr. J. L. Corish states that an extended series of experiments in the employment of ether, chloroform, nitrous oxide and oxygen, lead him to the following conclusions:

"(a) The excitement stage due to the cutting off of the oxygen from the circulation, thereby causing nervous reflex muscular movements and irritation of the air passages, is abolished when the oxygen is added, on account of the blood receiving sufficient oxygen.

"(b) The cyanosis which is caused by the reflex paralysis of the vaso-motors, thereby allowing dilatation of the venous and contraction of the arterial blood vessels, due to a lack of oxygen at the nerve centers, is little marked or entirely absent.

"(c) That there is no increase of the mucous secretions is due to the removal of the cause of irritation and congestion of the mucous membranes. This nuisance, which in many instances is intolerable, particularly in the surgery of the nose and mouth, has in some cases in which the secretion entered the larynx caused dangerous symptoms of asphyxiation or subsequent pneumonia.

"(d) The vomiting and nausea, owing to the congestion of the stomach and irritation of the palate, are alleviated and this of necessity does away with the danger of food becoming lodged in the air passages.

"(e) The anæsthesia may be continued without stertor. The symptom, due to muscular paralysis of the palate, is not a necessary accompaniment of anæsthesia. It shows that excess of the anæsthetic is being used. The palate is controlled by both voluntary and involuntary forces. This symptom is a very good guide for the operator to go by.

"(f) The recovery from anæsthesia is quicker and more complete, owing to a minimum of ether being used. The recovery cannot be hastened by the employment of oxygen separately after the operation.

"(g) The amount of ether used is just sufficient to keep the patient under its effects, and when thoroughly mixed with the oxygen (compound) no serious symptoms can result. The patient will not breathe at all if oxygen is given to the point of saturation, and therefore no more ether will be taken in than is required until the respiratory center calls for more oxygen.

"(h) Owing to the amount of oxygen stored up in the system by this method, the etherization may be discontinued at times for from fifteen to thirty minutes and complete anæsthesia may be readily and quickly reinduced in thirty seconds, if occasion requires, by application of the previous method. This advantage to the specialist in pharyngeal operations must be apparent.

"(i) Owing to the character of the heart beat not being much altered, the combined anæsthetic can be given with comparatively less danger in cases of stenosis and insufficiency of the cardiac valves.

"Finally, I wish to state that I believe and think it will be borne out by subsequent experiments that when the oxygen is given in superabundance in connection with ether, a double effect will be produced, i. e., an anæsthesia from ether primarily and from oxygen secondarily. Ether is eliminated from the system by means of the lungs through respiratory efforts. If we can supply sufficient oxygen to the system, so that the respiratory center is not irritated or rendered dormant, we prolong our anæsthesia until such time as the ether is split up chemically and passed off through the skin. Another point to be observed in the use of oxygen with ether is this: In extensive operations in which great loss of blood is to be expected and it is usual to constrict the limbs, it would be advisable to administer oxygen before the tourniquet is applied, so that when the blood is allowed to re-enter the circulation it will be in a condition more in conformity with that of the rest of the body.

"With regard to the anæsthetic power of oxygen when properly induced, in its application to surgery, I am positive that it will be of very great value in operations on young children who may require surgical interference of short duration. Operations about the mouth and nose could be performed to the entire satisfaction of the operator, as the apparatus could be dispensed with immediately on the cessation of respiration.

Under the condition of oxygen anæsthesia, so to speak, the system is saturated with oxygen, the blood in the veins assumes an arterial hue, and the surgeon would have difficulty in recognizing venous from arterial hæmorrhage, but this would be insignificant in comparison to the benefits obtained."—Am. Medical Review.

SURGERY—DEATH AFTER EXPLORATORY PUNCTURE OF HYDATID CYSTS.

Chauffard (Sem. Méd., August 8, 1896), records a case in which an exploratory puncture was made to confirm the diagnosis of hydatid cyst of the liver. Every antiseptic precaution was observed, and the fluid withdrawn was characteristic and clear. No pain was complained of. A few moments later the patient felt suddenly ill, intense irritation of the skin was experienced; this was followed by two severe epiletoid seizures. The mouth became filled with foam, and death ensued from cardiac collapse about twenty-five minutes after the operation. Post-mortem examination proved, apart from the liver, entirely negative. The heart was contracted in systole and empty. There was no effusion of liquid into the peritoneum, and no sign of inflammation of serous membranes. The liver was very large, and after removal of the cystic cavities weighed 2,450 g. Compensatory hyperplasia has frequently been noticed in these cases. During life the liver had performed its functions well, and there was no glycosuria or urobilinuria. Post-mortem no cirrhotic or sclerotic lesions were present; the hepatic cells were well preserved. The trabecular arrangement was disturbed, the glandular element having obliterated the capillary network, and an almost exclusively cellular parenchyma remaining. This is probably a condition identical with that of the foci of nodular hyperplasia observed in the subjects of tuberculous, syphilitic, or malarial disease. Death after puncture, which is rare, usually results in cases of intra-peritoneal rupture. Two theories only can account for this case: 1. That the symptoms were reflex, and of traumatic origin. 2. That they were due to intoxication by a special hydatid poison. The latter is the generally accepted, and, in this case, the only possible view. The three groups of symptoms—cutaneous, cerebro-spinal, and myocardiac—which are frequently met with after puncture of hydatid cysts can hardly be due to an ordinary traumatic reflex; there is obviously some reflex connected with the puncture of the cyst itself. Death through hepato-cardiac reflex was negatived in this case by the fact that the heart was arrested in systole. In the most attenuated form of hydatid intoxication—urticaria—patients are found to be very differently affected, and individual idiosyncrasy, with regard to the poison resulting from effusion into the peritoneum of a few drops of the fluid is the only way of accounting for the terrible effects produced in this case.—British Medical Journal.

THERAPEUTICS.

The following is recommended by Dr. Whitla as a remedy for baldness. Take of

Oil of rosemary	4 drachms.
Liquor epispasticus	2 drachms.
Oil of sweet almonds	1½ ounces.
Spirits of camphor	2 ounces.
Glycerine of borax	1 ounce.
Otto of roses	8 drops.
Tincture of jaborandi	1 ounce.

Mix well. "A little to be rubbed into the roots of the hair every night."

A new test for sugar in the urine, devised by Dr. A. R. Elliott, and claimed to be the most accurate, is as follows:

Solution 1.

℞ Cupric sulphate	27	grains.
Glycerine (pure)	3	drachms.
Distilled water	2½	"
Liq. potassæ, ad	4	ounces.

Dissolve the cupric sulphate in the glycerine and water, and gently heat. When cold, add the liquor potassæ.

Solution 2.—A saturated solution of chemically pure tartaric acid.

These solutions are quite stable, and keep indefinitely.

Into a test tube pour a drachm of the cupric acid solution. Boil gently over a spirit lamp. Then add two or three drops of the tartaric acid solution, and boil again. The urine is now added slowly drop by drop, until eight drops are added. If no reaction takes place by this time there is no sugar. The reaction is a yellowish or reddish or greenish gray deposit of sub-oxide, which is marked and unmistakable. In a few minutes the reaction deepens.—Philadelphia Medical and Surgical Reporter.

THE FUNCTIONS OF THE STOMACH.[1]

The great progress which has been made in the study of the chemical processes of digestion has not been attended by a corresponding progress in the therapeutics of indigestion, probably for three reasons.

1. Because too much reliance has been placed upon pharmaceutical preparations as a means of correcting the morbid conditions of the stomach.

2. Because of insufficient attention paid to other functions of the stomach than those concerned with the chemical processes involved in the liquefaction of the food.

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[1] Modern Medicine.

3. Because of neglect to consider the fact that proper liquefaction of food in the stomach requires the conversion of the starch by salivary digestion as well as the solution of proteid matters by the gastric juice.

Indeed, from numerous observations made by the writer, the conclusion seems just that the failure of proteid digestion in the stomach is much less likely to give rise to disordered digestion than the imperfect digestion of starch. It is probable for this reason that an exclusive meat diet, the milk diet, and other dietaries excluding starch have won so much reputation in the treatment of chronic indigestion. These exclusively nitrogenous dietaries are most successful in cases of hyperpepsia, although these are the very cases in which, from a physiological standpoint, they would seem to be least indicated.

It is in hyperpepsia also that the inconveniences arising from the indigestion of starch are most likely to occur. The fermentation of starch is an acid fermentation. This may occur in connection with hyperpepsia, while the fermentation of proteids is hindered by the presence of an abundant quantity of hydrochloric acid in the stomach. Hence we find proteids fermentation most frequent in hypopesia, and acid fermentation most frequent in hyperpepsia.

Professor Moritz, of Munich, in a recent lecture, made a number of interesting points, some of which are almost revolutionary in their significance. They may be summarized as follows:

1. The digestion of animals—at least, dogs—may be maintained after the removal of the stomach, as is shown by the experiments of Czerny and Kaiser. This agrees with the observations of Noorden, Ewald, and others, upon human beings, to the effect that good health may be maintained notwithstanding the existence of almost complete apepsia, the liquefaction of food in these cases depending upon the action of the saliva in converting the starch into dextrin and sugar. This change is often found to be complete in cases of apepsia, according to the writer's observation.

2. Von Mering, Moritz, and Brandl have shown by experiments that the stomach cannot be regarded as an organ for absorption, except for alcohol. Since alcohol is an artificial and not a natural product, and never intended by nature to serve as a food, it would appear from these experiments that the stomach cannot be regarded as an organ of absorption, except with relation to morbid and toxic substances.

3. The result of this fact is that the food-substances of the stomach are kept in a diluted condition, and thus prepared for easy entrance into the intestines, and for ready absorption after their passage from the stomach into the lower portion of the digestive tract. Röhmann has observed that concentrated solutions of sugar are not readily absorbed in the intes-

tine, and that solutions having a strength of 5 per cent. are most readily absorbed.

4. Quincke has shown that equalization of temperature takes place readily in the stomach, and that the stomach is not much disturbed by considerable extremes in temperature, although this is not true of the intestine. Bunge has shown the importance of the antiseptic function of the stomach through the influence of hydrochloric acid. We showed in a paper recently read before the American Academy of Medicine that the stomach itself is an organ of antisepsis, independent of the hydrochloric acid which it contains. Professor Moritz goes a little further, and claims for the stomach that its chief purposes are: (1) To serve as a reservoir in which the food is liquefied and its supply to the intestine regulated, thus favoring complete digestion and absorption. (2) Protection of the stomach against bacterial, chemical, and thermal causes of injury by disinfection, by dilution, and by the modification of temperature.

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MISCELLANEOUS.

SUTURE OF THE HEART.

Cappelen (Norsk Magazin for Lœgevidenskaben, March, 1896), reports the following case: A man aged 24 had some hours before admission received a stab from a knife in the left side. He went home alone, and about an hour afterward was found lying in a pool of blood. He was brought to the hospital in a cab, and on admission was found to be unconscious; the pulse could not be felt, but pure, though weak, heart sounds could be heard to the right of the sternum, on a level with the fourth rib; the impulse could not be felt. In the fourth left intercostal space, in the middle axillary line, parallel with the rib, was punctured, non-bleeding wound 1 cm. long. After a camphor injection the patient began to breathe and the pulse could be felt. The left side of the chest did not move in respiration. Under chloroform narcosis a resection of the fourth rib was made after enlarging the wound. The pleural cavity was filled with partly liquid, partly coagulated blood, compressing the lung. After evacuating the blood, which was estimated to be about 1,400 c.cm., the lung dilated and was found not to be wounded. By resecting 5 cm. of the third rib a wound 1 cm. long could be seen on the pericardium, bleeding freely. The sac was filled with coagula, and on enlarging the opening a wound 2 cm. in length was seen on the left ventricle, causing the bleeding. The wound was sutured and an artery tied, after which the hæmorrhage ceased. The needle was brought half way through during a contraction and then dropped, and when the heart dilated after a second contraction the point was grasped and the needle brought completely through. The suturing was made very difficult by the rythmic movements of the lung, which covered the whole operating field, and by the heart contractions, which, however, were perfectly regular and quiet all the time. The pericardial cavity was emptied of clots as far as practicable. The pulse after the operation was very quick and feeble, but improved after a sub-cutaneous saline injection. The patient sank gradually, however, and died two and a half days after the operation. At the necroscopy it was found that a large branch of the coronary artery had been wounded; the wound had begun to heal, but there was evidence of pericarditis, and various bacteria were found in the fibrous exudation. The knife had passed through the pleura in front of the lung without wounding it, and again through the pleura and pericardium into the heart.—British Medical Journal.

PREGNANCY DIAGNOSED BY THE URINE.

William E. Parke, following Dr. William D. Gray, of Richmond, states in the American Gynæcological and Obstetrical Journal that he can make a positive diagnosis of pregnancy within twenty days after conception, by certain changes in the microscopical appearance of the urinary phosphates. The normal triple phosphate is stellate and markedly feathery. Soon after conception the feathery parts begin to disintegrate, take on crystals, approach to normal, and at term are normal. In preparing the urine for examination Dr. Gray takes about one inch in a test tube and adds about one-third as much of Tyson's magnesian fluid. This will throw down the triple phosphates in fifteen or twenty minutes and furnish the necessary material for examination. Tyson's fluid consists of one part each of muriate ammonium, aqua ammonia, and sulphate of magnesium and eight parts of distilled water. When conception occurs the triple phosphates lose their feathery appearance, the change beginning at the tip and progressing toward the base. One side only may be affected, or both, leaving only the shaft and perhaps a few fragments adhering. The shaft assumes a beaded or jointed appearance. These changes are most marked in the early months of pregnancy. Dr. Gray draws the following conclusions:

1. The change occurs in a very large percentage of pregnant women.

2. This change is not equally pronounced in the urine at the same period of gestation in different women nor at consecutive examinations of the urine of the same woman.

3. When recognized it forms strongly presumptive evidence of pregnancy. This sign is recognizable very early (Dr. Gray, in a personal

letter, states that he has made many diagnoses as early as ten days after conception). It is, therefore, of the greatest value when other signs are of the least value, or not present at all.

4. A diagnosis of probable pregnancy can be made without a physical examination or without exciting suspicion of the patient.—New York Medical Record.

Photographs of the Human Voice.—William Hallock, Professor of Physics in Columbia College, has long been experimenting with the hope of obtaining pictures of musical sounds by means of the camera. It is well known that every vibrating body has a certain key note or pitch to which it vibrates, and it will not respond to any other note. Professor Hallock's apparatus consists of a series of hollow metallic spheres; each sphere is of a different size and each vibrates to a different musical tone. Each ball has a small hole in one side of it, and opposite the hole is placed a membranous drum-head. On the farther side of the drum is placed a lighted gas jet. All this is so arranged that when the air inside the ball is made to vibrate by the musical sound corresponding to its tone pitch, the membrane also vibrates, and this vibration is communicated to the gas light and causes it to jump.

The professor has arranged a series of these spheres, drums and lights, so that each one responds to a tone of the musical scale. When a singer strikes "C" the sphere vibrates, and so on through the gamut. A totally deaf person looking on could tell the tones sung by merely watching the gas jets.

As is well known to musicians and physiologists, the pitch of a tone depends on the number of vibrations, or the rapidity with which the sound waves follow each other. The loudness of a tone depends on the amplitude of the sound waves. In the human voice the more powerful the air blast the greater the amplitude of the vibrations, and hence the louder the sound produced. The quality of any musical tone depends on the relative power of the fundamental tone and that of the overtones or harmonics which accompany it. The less the fundamental tone is disturbed by overtones, the clearer and better is the quality of the voice. For example, when a bass voice sings the syllable "ah" to the pitch of bass "C," this tone "C" may be accompanied by the harmonics middle "C" and "G," and by the treble "C," "E," "G," "B flat"and high "C."

We will now return to Professor Hallock's invention. The bass singer seats himself before the apparatus and sings "C." He immediately sees from the jumping of the gas jets what overtones are present, and by counting these and then comparing the length of the jump of the jet corresponding to the fundamental tone "C" with the length of the jump of the jets corresponding to the overtones, he gets an idea of the quality of his voice. The power of his voice is indicated by the size of the jump of the gas jet of the fundamental tone "C." The camera is then brought in and the gas jets photographed, and thus a complete perfect record of the power, purity and sweetness of the singer's voice is obtained. What next?

This invention cannot fail to be a great boon to those benighted individuals who assume to serve upon the music committees of many of our churches, and whose only fitness for the position lies in the fact that they know nothing whatever about music. Applicants for positions in the church choir should now be expected to present a series of photographs for inspection of the Committee on Music. By such a plan the quality and compass of a singer's voice is shown at a glance, and even the unmusical can form some estimate of the applicant's fitness for the position. It is perhaps needless to add, that even this invention can give no indication of whether or no the singer possesses that subtle, indescribable charm which we call expression, or feeling, or sympathy or soul—and which is the very essence of all music—but it is not well to expect too much of the modern church choir singer.—Editorial, Journal of Medicine and Science.

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NEWS AND NOTES.

The Laryngoscope, published in St. Louis, has been selected as the official organ, for the year 1897, of the Laryngological Section of the New York Academy of Medicine. This selection, and the great probability of the same journal being chosen by other laryngological, rhinological and otological societies as their official organ, would indicate that the Laryngoscope had become what its proprietors stated they intended to make it, *i. e.*, the American journal of record for the specialties represented.

EUCAINE HYDROCHLORATE.—We quote from the American Year-Book of Medicine and Surgery, Gould, 1897, Department of General Surgery, by W. W. Keen, M.D., and John Chalmers DaCosta, M.D., of Philadelphia, the following:

"Trial has been made of eucaine in the out-patient surgical department of Jefferson Hospital, and the results have been most satisfactory. In the removal of ingrown toe-nails we have a procedure which brings a local anæsthetic to the test, and in these usually painful operations eucaine has proved more satisfactory than cocaine, the anæsthesia from eucaine being more profound and prolonged and widespread than is that from cocaine." (See Dr. Geo. W. Spencer's report in the University Medical Magazine, November, 1896).

Nestor Tirard, M.D., London, F.R.C.P., Professor of Materia Medica and Therapeutics, King's College, London, Physician to King's College Hospital, and to the Evelina Hospital for Sick Children, in his "Summary of the Therapeutics of the Year 1895-96," says on page 445 of the "Year-Book of Treatment," 1897, published by Messrs. Lea Bros. & Co., 708 Sansom street, Philadelphia, as follows:

"The new remedies which claim attention are not so numerous as usual, and with the exception, perhaps, of eucaine, it cannot be said that they present any striking characteristics."

The Forty-eighth[1] Annual Session of the American Medical Association will be held in Philadelphia, Pa., on Tuesday, Wednesday, Thursday and Friday, June 1, 2, 3 and 4, commencing on Tuesday, at 10 A. M.

"The delegates shall receive their appointment from permanently organized State medical societies, and such county and district medical societies as are recognized by representation in their respective State societies, and from the medical departments of the Army and Navy and the Marine-Hospital Service of the United States.

"Each State, county, and district medical society entitled to representation shall have the privilege of sending to the association one delegate for every ten resident members, and one for every additional fraction of more than half that number; Provided, however, that the number of delegates for any particular State, territory, county, city or town shall not exceed the ratio of one in ten of the resident physicians who may have signed the Code of Ethics of the association."

Members by Application.—Members by application shall consist of such members of the State, county and district medical societies entitled to representation in this association as shall make application in writing to the treasurer, and accompany said application with a certificate of good standing, signed by the president and secretary of the society of which they are members, and the amount of the annual subscription fee, $5. They shall have their names upon the roll, and have all the rights and privileges accorded to permanent members, and shall retain their membership upon the same terms.

The following resolution was adopted at the session of 1888:

That in future each delegate or permanent member shall, when he registers, also record the name of the Section, if any, that he will attend, and in which he will cast his vote for Section officers.

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[1] There were no meetings held by the association during the years 1861 and 1862.

Secretaries of medical societies, as above designated, are earnestly requested to forward, at once, lists of their delegates.

Addresses: The presidential address, Nicholas Senn, Chicago; address in surgery, Wm. W. Keen, Philadelphia; address in medicine, Austin Flint, New York; address in State medicine, John B. Hamilton, Chicago. Committee of Arrangements: H. A. Hare, 222 S. 15th street, Philadelphia.

The fifth annual meeting of the Tri-State Medical Society of Iowa, Illinois and Missouri, will meet in St. Louis, April 6, 7 and 8, 1897. A large number of valuable papers will be read. Dr. Joseph Price, of Philadelphia, wil hold the surgical clinic; Dr. James T. Whittaker, of Cincinnati, the medical clinic, and Dr. Dudley Reynolds, ophthalmic clinic. Dr. G. Frank Lydston, of Chicago, will entertain the members with an original story during one of the evening sessions. The officers are: A. H. Cordier, M.D., president, Rialto Building, Kansas City; Hugh T. Patrick, M.D., 1st vice-president, Chicago; H. C. Eschbach, M.D., 2d vice-president, Albia, Ia.; G. W. Cale, M.D., secretary, 4403 Washington boulevard, St. Louis; C. S. Chase, M.D., treasurer, Waterloo, Ia.

The Centennial and International Exposition to celebrate the one hundredth year of statehood by the citizens of Tennessee is now an established fact, and the opening day is fixed for Saturday, May 1, prox. Already the sum of over $500,000 has been subscribed by the citizens of the State alone, most of which has been very economically and judiciously expended in the erection of large, commodious, elegant and suitable buildings, and in the ornamentation of the grounds. An additional $130,000 has been appropriated by Congress for a special government exhibit, the State Legislature now in session will undoubtedly appropriate $100,000 has been appropriated by Congress for a special government exhibit; special exhibits; and other states and cities are taking active and important steps to have a full representation before the large multitudes of people from all parts of the world who will visit the Capital City of the Volunteer State at some time between the 1st of May and the 31st of October next.

A separate and special building for medical and surgical appliances and hygiene will be provided for everything pertaining to these sciences and arts. This building will be specially attractive to the many medical men who will be in attendance from all parts of the world at some time during the season. A dozen or more prominent medical organizations have signified their intention of meeting in Nashville during the centennial, as well as many doctors who are in affiliation with other orders, societies and organizations that will meet here.

The committee in charge of Section "B,," Medical History and Literature, beg leave to respectfully suggest that you keep a special file or files of your periodical from the first of the current year until the opening day, to be placed in the space allotted, and that you send one or more copies regularly for inspection by the visitors during the progress of the exposition. Should you deem proper to avail yourself of this opportunity of bringing your publication to the notice of our visitors, an early communication to that effect will be appreciated. At a later date, full instructions will be given as to when and how this special file and subsequent issues of your publication may be sent.

Very respectfully and truly yours,
Deering J. Roberts, Chairman.

Nashville, Tenn.

Preliminary programme for the fourth session of the Congress of American Physicians and Surgeons, to be held in Washington, D. C., May 4, 5 and 6, 1897.

President, William H. Welch, M.D., LL.D., Baltimore, Md.

Vice-presidents, ex-officio, the president of the American Otological Society, Dr. Arthur Mathewson, Brooklyn, N. Y.; the president of the American Neurological Association, Dr. M. Allen Starr, New York City; the president of the American Gynæcological Society, Dr. James R. Chadwick, Boston, Mass.; the president of the American Dermatological Association, Dr. James C. White, Boston, Mass.; the president of the American Laryngological Association, Dr. Charles H. Knight, New York City; the president of the American Climatological Association, Dr. E. Fletcher Ingals, Chicago, Ill.; the president of the Association of American Physicians, Dr. J. M. DaCosta, Philadelphia, Pa.; the president of the American Association of Genito-Urinary Surgeons, Dr. Francis S. Watson, Boston, Mass.; the president of the American Orthopedic Association, Dr. Samuel Ketch, New York City; the president of the American Physiological Society, Dr. Russell H. Chittenden, New Haven, Conn.; the president of the Association of American Anatomists, Dr. Frank Baker, Washington, D. C.; the president of the American Pediatric Society, Dr. Samuel S. Adams, Washington, D. C.; the president of the American Surgical Association, Dr. John Collins Warren, Boston, Mass.; the president of the American Ophthalmological Society, Dr. George C. Harlan, Philadelphia, Pa.

Chairman of the Executive Committee, Landon Carter Gray, M.D., New York City.

Treasurer, Newton M. Shaffer, M.D., New York City.

Secretary, William H. Carmalt, M.D., New Haven, Conn.

Programme.—The meeting of the Congress will be held in the Columbia Theatre, corner of Twelfth and F streets, N. W.

Tuesday, May 4, a business meeting of the Congress will be held from 1.30 to 2 P. M. From 2 to 3.30, a general meeting of the Congress under the direction of "The American Ophthalmological Society, subject: "The Gouty and Rheumatic Diatheses, and Their Relation to Diseases of the Eye." Papers will be read by Dr. Charles Stedman Bull, of New York City; Dr. S. Oliver Richey, of Washington, D. C.; Dr. S. D. Risley, of Philadelphia, Pa.; Dr. Robert Sattler, of Cincinnati, O., and Dr. R. A. Reeves, of Toronto, Can.; to be followed by a discussion in which Dr. J. M. DaCosta, of Philadelphia, Pa., and Dr. Henry M. Lyman, Chicago, Ill., members of the Association of American Physicians, and others, will participate. From 3.30 to 5 P. M., a general meeting under the direction of The American Otological Society, subject: "Otology in Its Relations to General Medicine." A paper by Dr. Clarence J. Blake, of Boston, Mass.

Wednesday, May 5, from 2 to 5 P. M., general meeting of the Congress, under the joint participation of The Association of American Physicians, The American Physiological Society, and The American Pediatric Society. Subject: "Internal Secretions Considered in Their Physiological, Pathological and Clinical Aspects." Dr. William H. Howell, of Baltimore, Md., and Dr. Russell H. Chittenden, of New Haven, Conn., will speak in behalf of The American Physiological Society; Dr. J. George Adami, of Montreal, Can., Dr. James J. Putnam, of Boston, Mass., and Dr. Francis P. Kinnicutt, of New York City, in behalf of The Association of American Physicians, and Dr. William Osler, of Baltimore, Md., in behalf of The American Pediatric Society. The papers will be followed by a discussion.

Wednesday, May 5, Evening Meeting, 8.15 P. M.—Address by the president of the Congress, Dr. William H. Welch, Professor of Pathology in the Johns Hopkins University, Baltimore, Md., to be followed by a reception by the president at Rauscher's, corner of Connecticut avenue and L street.

Thursday, May 6, from 2 to 3.30 P. M.—General meeting of the Congress, under the direction of The American Orthopedic Association. Subject: "Deformities of the Hip-Joint, Especially Congenital Dislocations." A paper will be read by Dr. E. H. Bradford, of Boston, Mass., to be followed by a discussion by Dr. V. P. Gibney, of New York City, and Dr. Harry M. Sherman, of San Francisco, Cal. From 3.30 to 5 P. M. —General meeting of the Congress, under the direction of The American

Surgical Association. Subject: "The Classification of Acute General Peritonitis, the Prognosis and Treatment of the Different Varieties." Dr. William S. Halsted, of Baltimore, Md., will read a paper on "Classification," and Dr. Robert Abbe, of New York City, on "The Prognosis and Treatment of the Different Varieties." A discussion will follow, participated in by Dr. John Homans, of Boston, Mass., Dr. A. Van der Veer, of Albany, N. Y., Dr. Henry H. Mudd, of St. Louis, Mo., Dr. Frederick Lange and Dr. Arpad G. Gerster, of New York City.

Programme of the Western Ophthalmological, Otological, Laryngological and Rhinological Association. Second annual meeting, St. Louis, Mo., April 8-9, 1897. Place of meeting: Planters Hotel.

President, Adolf Alt; vice-presidents, B. E. Fryer, J. H. Martindale; treasurer, W. L. Dayton; secretary, Hal Foster.

Programme, Thursday, April 8, session at 9 o'clock.—Address of Welcome by Dr. W. J. Langan, president St. Louis Medical Society.

Response by Dr. A. C. Corr, president Illinois State Medical Society.

President's address: "Hæmorrhagic Glaucoma"—Dr. Adolf Alt, St. Louis, Mo.

Reception of guests, registering and payment of dues, report of the secretary and treasurer, election of members.

A cordial invitation is extended to the profession. The railroads have promised one and one-third fare. When buying tickets ask for a certificate; on reaching St. Louis have it signed by Dr. Alt.

Presentation of papers:

1. "Report of Six Cases of Removal of Ossicles," Dr. Norval H. Pierce, Chicago, Ill.
2. "A Pyramidal and Senile Cataract in One Subject, Report of a Case," Dr. Geo. F. Suker, Toledo, O.
3. "A Case of Oculo-Motor Paralysis," Dr. Geo. E. Bellows, Kansas City, Mo.
4. "Adenoid Vegetation," Dr. Ellet Orrin Sisson, Keokuk, Ia.
5. "Some Observations Upon the Irritating Effects of Natural Gas Upon Trachoma," Dr. John Johnson Kyle, Marion, Ind.
6. "Treatment of Chronic Suppuration of the Middle Ear," Dr. S. S. Bishop, Chicago, Ill.
7. "Astigmatism," Dr. Dudley S. Reynolds, Louisville, Ky.
8. "Diseases of the Glosso-Epiglottic Space," Dr. J. F. Barnhill, Indianapolis, Ind.
9. "The Classification of the Exudative Anginæ," Dr. E. B. La Fevre, Abilene, Kan.

10. "Skin Grafting for Malignancy of the Orbit and Entropion," Dr. Flavel B. Tiffany, Kansas City, Mo.

11. "Restoration of the Eye Lids by Skin Grafting," Dr. W. C. Tyree, Kansas City, Mo.

12. "An Improved Skiascope," Dr. J. Ellis Jennings, St. Louis, Mo.

13. "Scopolamine as a Mydriatic and Cycloplegic," Dr. William S. Fowler, Chicago. Ill.

14. "Optic Neuritis," Dr. F. C. Evans, Louisville, Ky.

15. "Report of a Case of Congenital Membraneous Cataract with Aphakia," Dr. A. S. McGee, Topeka, Kan.

16. "Moderate Errors of Refraction: Shall We Always Correct Them?" Dr. Albert E. Bulson, Ft. Wayne, Ind.

17. "Conservatism in Rhinological Practice," Dr. B. M. Berens, Minneapolis, Minn.

18. "Mental Depression and Prolonged Melancholia Following Graduated Tenotomy and the Limitation of Prisms," Dr. W. H. Baker, Lynchburg, Va.

19. "Report of a Case of Double Mastoid Disease, Operations, Recovery," Dr. J. O. Stillson, Indianapolis, Ind.

20. "The Technique of Cataract Extraction," Dr. E. B. Fryer, Kansas City, Mo.

21. "Disease of Labyrinth," Dr. J. Holinger, Chicago, Ill.

22. "Keratoconus," Dr. J. W. Bullard, Pawnee City, Neb.

23. "A Case of Mastoiditis Complicating Purulent Otitis Media Dr. J. Aloysius Mullen, Houston, Tex.

24. "Syphilitic Amblyopia," Dr. Robert F. Lemond, Denver, Colo.

25. "Congenital Nystagmus," Dr. J. Elliot Colburn, Chicago, Ill.

26. "Objective Noises in the Ear," Dr. G. Sterling Ryerson, Toronto, Can.

27. "Function of the Stapedius and Tensor Tympani Muscles," Dr. Thos. F. Rumbold, St. Louis, Mo.

28. "Saline Injections in the Treatment of Cyclitis and Iritis," Dr. S. L. Ledbetter, Birmingham, Ala.

29. Paper, Dr. W. E. Gamble, Chicago, Ill.

30. "College Instruction in Ophthalmology," Dr. A. M. Lapsley, Keokuk, Ia.

31. "The Value of Hypnotic Suggestion in Ophthalmic Practice," Dr. Ignatz Mayer, Guthrie, O. T.

32. "Description of Dr. McCassy's Trial Frame, Lachrymal Style and Threaded Handle," Dr. J. H. McCassy, Dayton, O.

33. "The Lens Tester and Why Used," Dr. J. H. Johnson, Kansas City, Mo.

34. "The Etiology, Treatment and Prognosis in Exopthalmic Goitre," Dr. J. Fred Clark, Fairfield, Ia.

35. "Extraction of Bilateral Soft Cataract in the Case of a Child Three Years Old," Dr. J. O. McReynolds, Dallas, Tex.

Friday, April 9.—Election of members; election of officers.

Papers:

1. "Two Cases of Asthma Due to Intra-Nasal Growths," Dr. W. W. Bulette, Pueblo, Colo.

2. "Aural Pain," Dr. A. M. Howe, Wichita, Kan.

3. "Influence of Vaporized Medicaments on the Respiratory Passages," Dr. Homar M. Thomas, Chicago, Ill.

4. "Hysteria in Ophthalmology," Dr. W. L. Dayton, Lincoln. Neb.

5. "Otitis Media Circumscripta," Dr. C. M. Holcomb, Winfield, Kan.

6. "Ulcers of the Cornea," Dr. H. Z. Gill, Pittsburg, Kan.

7. Paper, Dr. J. D. C. Hoit, Elmwood, Ill.

8. Paper, Dr. Frank Allport, Minneapolis, Minn.

9. "Hypertrophic Rhinitis," Dr. W. T. Grove, Eureka, Kan.

10. "The Relative Value of Enucleation and Evisceration," Dr. A. R. Amos, Des Moines, Ia.

11. Paper, Dr. C. W. Kollock, Charleston, S. C.

12. "A Plea for More Mild Treatment of the Conjunctiva," Dr. E. W. Ames, Canton, Ill.

13. Paper, Dr. Geo. Knapp, Vincennes, Ind.

14. "Thrombosis of the Lateral Sinus," Dr. B. F. Church, Dallas, Tex.

15. "Notes on Nasal Surgery," Dr. A. E. Prince, Springfield, Ill.

16. Paper, Dr. K. K. Wheelock, Fort Wayne, Ind.

17. "Chronic Rhinitis as a Factor in Weakened Vision," Dr. Joseph A. Daniel, Davenport, Ia.

18. Paper, Dr. Charles H. Beard, Chicago, Ill.

19. "Treatment of Corneal Lesions by Hydraulic Curetting with Sublimate Solutions," Dr. C. H. Pleasants, Helena, Mont.

20. Paper and Demonstrations: "Advanced Methods in Teaching the Deaf," Dr. M. A. Goldstein, St. Louis, Mo.

Discussion opened by Prof. S. T. Walker, Jacksonville, Ill.

21. "Three cases of Suppuration of the Frontal Sinuses Treated by Means of the Air Douche," Dr. P. F. Gildea, Colorado Springs, Colo.

22. "Toleration of the Eye to Severe Injuries," Dr. H. G. Sherman, Cleveland, O.

23. "A case of Mastoiditis Complicating Purulent Otitis Media Cured by Enlarging the Drum Perforation and Syringing the Tympanic Cavity," Dr. Wm. Sheppegrell, New Orleans, La.

24. "The Relationship of Obscure Throat Symptoms in Adults to the Pharyngeal Tonsil," Dr. H. Moulton, Fort Smith, Ark.

25. "Suppurative Inflammation of the Frontal Sinuses," Dr. Frank E. Sampson, Creston, Ia.

26. "Experiments on the Eustachian Tubes by Means of the Tongue Thrust Into the Naso-Pharynx," Dr. Hamilton Stillson, Seattle, Wash.

27. "Atrophic Rhinitis," Dr. I. Cullen, Cincinnati, O.

28. "Artificial Membrana Tympani," Dr. E.W. Heltman, Toledo, O.

29. "Otitis Media," Dr. E. M. Singleton, Marshalltown, Ia.

30. Paper, Dr. F. E. Waxham, Denver, Colo.

31. "Throat Manifestation of Transmitted Syphilis," Dr. H. W. Whitaker, Columbus, O.

32. "Relative Value of the Various Treatments for Deafness Due to Otitis Media," Dr. J. F. Oaks, Chicago, Ill.

33. "Eustachian Catarrh," Dr. M. Jay Brown, Salina, Kan.

34. "Nitrate of Silver," Dr. Charles E. Walker, Denver, Colo.

35. Paper, Dr. J. H. Martindale, Minneapolis, Minn.

36. Paper, Dr. F. C. Heath, Indianapolis, Ind.

37. Paper, Dr. Francis B. Kellogg, Tacoma, Wash.

38. Paper, Dr. Allen T. Haight, Chicago, Ill.

39. "Lymphadenoma with Report of Cases," Dr. C. W. Parker, St. Louis, Mo.

40. Paper, Dr. D. Emmett Welsh, Grand Rapids, Mich.

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REVIEWS.

The Year Book of Treatment for 1897. A critical review for practitioners of medicine and surgery. Lea Bros. & Co., Philadelphia and New York, 1897.

The editor quite appropriately observes in the first paragraph of his preface to this volume that "This being the thirteenth issue of the present painstaking and brief chronicle of the therapeutic work of the past twelve months, 'The Year Book of Treatment' may now be considered to have taken it place among the 'hardy annuals' of medical literature." We have had occasion so often to refer to the characteristics of this little volume, that it is hardly necessary to do more now than recall the fact that we find it, of all the books of its kind that come to our notice, one of the most helpful, practical and successful. While the brevity of some of the articles may, in a measure, impair their usefulness, the succinctness and the general thoroughness with which the subject of medicine and surgery is reviewed makes this little volume pre-eminently practical to the active worker.

The Diseases of the Stomach. By Dr. C. A. Ewald, Extraordinary Professor of Medicine at the University of Berlin, etc. Translated and edited with numerous additions, from the third German edition, by Morris Manges, A.M., M.D., Assistant Visiting Physician to Mt. Sinai Hospital, etc. Second revised edition. New York, D. Appleton & Co., 1897.

As this is the second American edition of Dr. Ewald's well-known treatise on diseases of the stomach, it is only necessary to call attention to the fact that so much progress has been made in the diagnosis and treatment of diseases of the stomach since the appearance of the first edition of this work, that another edition has been necessary. The present American editor is unusually well fitted for his task, both by an intimate knowledge of the subject and by personal knowledge of the original language in which the work was written. It may seem strange, but it is unquestionably the fact, that several works written in a foreign language have within the past few years been published in the English language and edited by men who were ignorant of the language in which they were written, and who were, therefore, entirely unequal to the task. The greatest advance has probably been along the lines of diagnosis, especially of the diagnosis of cancer. Unfortunately, the treatment has not kept pace with this advance in diagnosis; and yet it is surprising to find that the mortality per cent. in pylorectomy cases from the year 1890 to the year 1895 has changed from 55.1 to 26.6; and the mortality in cases of gastro-enterostomy, according to the latest statistics of Hahn and Rochwitz, is only 12½ per cent. Apropos of this, the editor remarks: "The respective indications, according to the present views, are that, taken all in all, gastro-enterostomy offers the best chance for pyloric cancer."

Another line in which there has been decided improvement and advance, is the surgical treatment of perforating ulcers. According to a recent paper by Weir and Foote, the mortality of 23 cases operated within 12 hours was only 29 per cent., while in 17 cases operated within 12 to 24 hours it was 76 per cent., and in 32 cases operated after 24 hours it was 87 per cent. With the advance and improvement in the technique of all surgical procedures, we may confidently look forward to a decided improvement even upon these statistics. Where the diagnosis of perforation can be made with any degree of certainty, early operative treatment unquestionably offers the best chances for a successful issue.

Naturally, the whole series of questions connected with the diseases of the stomach is considered in a way that cannot be done in the ordinary books on general medicine; and it is not surprising, in view of the unlimited disturbances of this most important organ, that so many special works are appearing on the subject, all of which receive a fair amount of patronage.

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All business communications should be addressed to Gaillard's Medical Journal, P. O. Box 1124, New York. All editorial communications to Carter S. Cole, M.D., 101 W. 74th St., New York.

Articles for publication will be received with the understanding that they are contributed exclusively to this journal. Reprints will be furnished at actual cost, orders for which should accompany the manuscript. Authors of accepted articles may receive twelve copies of the issue in which they are published. Necessary illustrations will be furnished without expense to authors when suitable drawings or photographs are furnished.

EDITORIALS.

CREDIT FOR ARTICLES.

In our March issue the printers failed to give credit for two of the selections. The first, "Lectures on Angina Pectoris and Allied States," was from the New York Medical Journal, in which it originally appeared. The other, "Ideal Catgut Sterilization," appeared in the Medical Mirror, of St. Louis. We are always grieved to have an omission of this sort, as it has been an abiding principle of the JOURNAL to give credit for even the shortest piece taken from another source. While we are especially sorry, the fact that both of these were put under the head of "Selections" shows that the failure to properly credit them was a fault of omission and not of commission. And while we are acknowledging our own shortcomings, it may not be amiss to call attention to the fact that it is by no means unusual for some of the journals to fail in the very way in which they are the first to complain. An article may possibly not be reproduced verbatim, but the substance of it is frequently given without any reference at all to the original source. It may be difficult at times for subscribers to understand why an article furnished expressly under the condition that it shall be offered as an original article to no other journal, appears in several. The matter is simple. The author furnishes the copy to several journals, telling each one that it has not yet appeared and that he is at liberty to use it as an original article. Such was the case with an article we recently published, which, to our great amazement, has appeared in no less than six or eight journals as an original article, while we hold the author's letter making statement as given above. It is true that at the time it was offered to us for publication it had not appeared, but it is perhaps none the less true that it was sent to the other journals at the same time.

But there is another aspect to the case. There is really no reason why, as is done constantly with very important public lectures and series of articles by prominent English physicians and surgeons, copy should not be furnished to one or more journals for reproduction. The only substantial recompense to the author is the advertisement, and if this is the case, it would seem but fair that he should have as much of this as possible. This may make it difficult at times to properly credit the original source, but where several sources are available, the obligation still remains to credit the source from which the aritcle was obtained. We shall endeavor in the future, as we have done in the past, to accurately account for all matter borrowed.

THE UNDIMINISHED MORTALITY FROM PUERPERAL FEVER IN ENGLAND AND WALES.

In an inaugural address with the above title, delivered before the Obstetrical Society of London, on March 3, 1897, by Chas. J. Cullingworth, F.R.C.P., London, president of the society, we are amazed to find this statement: that the figures of the registrar general showed that, so far from having diminished, the death rate from puerperal fever in England and Wales had considerably increased. The author goes on to say that "Although puerperal fever has practically become a thing of the past in our lying-in hospitals, it is impossible to ignore the fact that there has not been a corresponding diminution of mortality from puerperal fever in ordinary private practice. It was not to be expected that those who had already been engaged for some time in practice would at once assimilate the new ideas and adopt what appeared to many of them not only new but revolutionary and unnecessary methods; but as time wore on and a younger race of men came upon the scene, it was not unnaturally hoped that an improvement would take place and that a mortality which had been conclusively shown to be preventable, would show some signs of diminution. * * * * Do we teachers sufficiently impress upon the minds of our students the infinite importance of this subject? Are we ourselves careful to show by our example the importance of the precautions that we lay down in the lecture theater? * * * * * A great deal has been written on the superiority of asepsis as compared with antisepsis, and it has become the fashion in some quarters to speak of antiseptics as though they had their day, and to maintain that a condition of asepsis can be maintained by cleanliness alone. It is well, I think, to bear in mind that the triumphs of our lying-in hospitals have all been won by the scrupulous use of antiseptics, and sterilization, which is an essential element in aseptic surgery, is impossible in midwifery practice. I firmly believe that if the simple antiseptic precautions with which every one is familiar were conscientiously adopted, puerperal fever would be as rare in private practice as it is now in the best lying-in hospitals. The only way, as I have elsewhere observed, to avoid the present terrible mortality and to avoid also the terrible amount of puerperal disease, which, because it is not fatal, is apt to remain unrecorded, is for every practitioner in midwifery to recognize his personal responsibility in the matter." He then quotes a woman who says: "Now, I maintain that women themselves have a personal responsibility in this matter also. They should not be content passively to allow their lives to lie in the hands of others who may or may not choose to use antiseptics. Women who are expecting their confinement should satisfy themselves beforehand that the doctor and the nurse engaged to attend will concientiously use these safeguards."

It is rather remarkable for us to have this matter presented in such a light at this late day. The strides made in the hospitals in this country have certainly been fairly well followed in private practice; and while we are not in possession of accurate statistics from outlying districts, we are comfortably assured by the large mass of general practitioners that puerperal fever has rapidly become a thing of the past. The teaching of this country has been for a number of years that no man can escape the imputation of personal responsibility for such a condition of affairs. If he assumes the care and conduct of a puerperal case, then upon him devolves the responsibility of the prevention of such dire calamity. When we look back upon the valuable lives sacrificed upon the altar of filth, ignorance and carelessness, it does seem that the least that can be required of a practitioner to-day is to employ the most thorough, careful, conscientious detail in the conduct of every puerperal case; and if he will recognize at the outset that he alone, in the vast majority of cases, if not in every case, is responsible for puerperal sepsis, the disease will soon become so rare that he shall forget how to treat it. As to the actual means to be employed, this is neither the time nor the occasion to consider them, except to say that absolute cleanliness—surgical cleanliness—is a distinct advance towards the complete prevention of puerperal sepsis. And the fact that this cleanliness can be obtained without antiseptics, does not alter the fact, as stated in the quotation above, that the results have been obtained by antiseptic procedures. One more fact we may add. We do not believe that a practitioner who is attending a case of contagious disease has any right to jeopardize the life of a woman by attending her in confinement at the same time.

NATIONAL CONFEDERATION OF STATE MEDICAL EXAMINING AND LICENSING BOARDS.

Officers, 1897: President, Wm. W. Potter, N. Y.; vice-presidents, Chas. A. L. Reed, O.; J. N. McCormack, Ky.; Secretary-treasurer, A. Walter Suiter, Herkimer, N. Y. Executive council, Perry H. Millard, St. Paul; Jos. M. Hathews, Louisville; Wm. S. Foster, Pittsburg; Hugh M. Taylor, Richmond; Jas. M. Hays, Greensboro, N. C.

Preliminary announcement of the seventh annual meeting. Office of the president, 284 Franklin St., Buffalo, N. Y., March 15, 1897.

Dear Doctor:—The seventh annual meeting of this confederation will be held in the small banquet hall of the Hotel Walton, at Philadelphia, Monday, May 31, 1897, at 10 o'clock, A. M. The following programme has been arranged:

I. Address of welcome by A. H. Hulshizer, of Penn., St. Bd. of Med. Exam.
II. Response by Vice-President Reed.
III. Report of the committee on minimum standard of requirements.
IV. Discussion and action thereon.
V. Report of the secretary and treasurer.
VI. Annual address of the president.
VII. Some practical experience with, and results of, the medical law of Pennsylvania. Wm. S. Foster, Pittsburg.
VIII. The need for exact information as to the equipment, methods, and requirements of our medical schools. J. N. McCormack, Bowling Green, Ky.
IX. Address by Prof. J. W. Holland, M.D., Dean Jefferson Med. Coll., Philadelphia.
X. Paper.
XI. Miscellaneous business.
XII. Election of officers.
XIII. Adjournment.

The object of the confederation is to consider questions pertaining to state control in medicine and to compare methods in vogue in the several states; the collection and dissemination of information relating to medical education, and to consider propositions that have for their purpose advancement of the standards in the United States. A cordial invitation is extended to all members and ex-members of state medical examining boards, and to physicians, sanitarians and educators who are friendly to the objects named, to attend the meeting and participate in its proceedings. By order of the Executive Council.

William Warren Potter, Prest.

A. Walter Suiter, Secy.

GAILLARD'S MEDICAL JOURNAL.

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ORIGINAL ARTICLES.

ACCIDENTS FROM EQUESTRIANISM.

By W. THORNTON PARKER, M.D.,
Late A. A. Surg., U. S. Army.

In the opinion of some very excellent military men, the lance and the sword are passing out of use. The terrible precision and destructive ability of modern arms are so great that cavalry charges will seldom be made use of in actual warfare.

Be that as it may, there are men of wide experience who will not believe that the days of cavalry service are numbered. Not only this, but they believe that the cavalry service will be developed and made use of for many years to come.

Surely in the war in Cuba, the use of cavalry by the insurgents has made a formidable impression upon the Spanish regulars.

During student days in Germany, I noticed that those of my countrymen who entered duel corps, or Burschenschafts, of students, soon became excellent swordsmen, in some instances making remarkable records.

But in this country comparatively little attention is paid to the sword, and few know how to wield one with dexterity.

The Americans are excellent shots with rifle or revolver, but are more or less indifferent to broadsword exercise.

Then the present bicycle craze has seized every one so firmly by the ears, and its friends would have us believe that the bicycle will take the place of everything, and carry everything, including babies and sweethearts.

I have spent too much time in the saddle to be willing to straddle a treadmill machine called a bicycle; and I believe that for pleasure even one hour on horseback is worth a day of cycling.

For years I have observed the opinions which, from time to time, have been expressed in our military and other journals, concerning the "passing of the horse."

I have been told that bicycling is so much safer than horseback riding.

I have tried very hard to secure statistics concerning accidents from equestrianism, but these are very difficult to obtain.

Neither the Surgeon-Generals of Armies or of Militia seem to consider equestrian accidents of sufficient importance to tabulate by themselves. The greatest interest in this subject has been shown by the military medical authorities of the Republic of Switzerland.

I have received some interesting letters from surgeons who are known to be excellent riders, and from gentlemen connected with riding clubs.

Some time since, I read an interesting paper by Dr. Hall, of Colorado, concerning accidents from equestrianism among cow boys.

In my experience I have never seen better riders, either among the European nations, or among Canadians, Americans, or Mexicans, than our American cow boys, or as they should be more properly called, "riders of the plains."

Among our American cavalry regiments, the horsemanship certainly equals that of any military organization I have ever seen; and, although I may seem to show partiality, the riding between the years of '67 and '77, of the 3d U. S. Cavalry, was in my opinion unequaled by that of any cavalry organization of England, Germany, France or Austria.

There is much to be said in favor of horseback riding for civilians.

A recent article in the *Illustrated American* on this subject is well worthy of notice. The writer of the article quotes, "The best thing for the inside of a man is the outside of a horse"—and from Lord Herbert of Cherburg, "A good rider on a good horse is as much above himself and the rest of mankind as the world can make him." "It is true," says the writer, "that the bicycle had not then come to its modern state of perfection and popularity as a means of individual locomotion, so some knight of the wheel may question the present truth of this statement. Nevertheless, I have yet to find a person who has really felt the sense of power, freedom, and exhilaration which comes from the consciousness of a thoroughly secure and 'at home' seat on the back of the noblest of all animals, who does not agree with Lord Herbert."

So far as my own experience is concerned, I cannot recall a single accident from real equestrianism which had a fatal result. The only accident which rendered the soldier utterly unfit for service was in the case of a sergeant of cavalry who volunteered to ride a powerful iron-gray cavalry horse of fiery disposition. No one had been able to conquer the animal, and this sergeant having had experience in the War of the Rebellion, asked permission to try his hand.

We were then on the march with a detachment of recruits and regular cavalry proceeding to New Mexico; and harassed as we were by daily desertions and by strong bands of hostile Sioux who forced their company upon us, we could ill afford to spare so good a soldier.

One afternoon, shortly after making camp, the command assembled to witness this extraordinary encounter between soldier and horse. The man succeeded in retaining his seat in the saddle after an exciting mount. The animal made every possible effort to unhorse the rider, but without avail. Finally, with one wild rush he threw himself upon the ground, and before the sergeant could extricate himself from the stirrups, the horse rolled over the man twice. He was brought to the hospital tent in a semi-conscious condition, and upon examination it was found that he had sustained an inguinal rupture on each side with a severe prolapse of the rectum.

The first equestrian accident I witnessed was in the vicinity of Fort Riley, Kansas, when the horse of a cavalryman stumbled and fell, throwing his rider violently onto the ground; from the effects of which fall he received a dislocation of the humerus of the left shoulder.

In my own experience, which has covered a great many miles of horseback riding, I have had two falls, one in attempting to mount a very spirited and restless animal, when I lost the stirrup and fell to the ground; another time in fording a river in Western Kansas, the banks being very steep and made slippery by reason of the numerous wagons, ambulances, and mounted men having dragged the water up onto the banks; when about half way up the slippery incline my horse lost his footing and fell heavily upon my left leg. But with a powerful spring he succeeded in gaining his feet again and in making the rest of the ascent safely.

So far as I can recollect, I have never attended a patient injured in the head from an accident of this nature.

Indirectly, cavalry service may cause injuries resulting from the pressure of the belt; such a case I reported in the *Philadelphia Medical Times*.

A soldier formerly of the Royal Life Guards of England, and later in our own cavalry, came under my care only a few weeks before his death and about eight months after he was taken ill.

I found him much emaciated, feeble, and anxious, complaining of severe pain and pressure in the abdominal region.

Upon examination, a large tumor was found occupying a considerable space in the epigastric, umbilical, and left hypocondriac regions. Although the treatment under which he was immediately placed seemed for a time to relieve his suffering, he suddenly became much worse and died. The autopsy revealed a large tumor in the abdominal cavity. The man had been in the habit of wearing a heavy saber, and, naturally of thin figure, the pressure of tightly buckled belt, and the sudden jerks of the saber on the belt in the violent military exercises *may* have been the origin of this fatal tumor. * * * *

We more or less frequently read in the newspapers of accidents happening to gentlemen, where the horse has made a sudden shy and thrown the rider. In falling, the head struck against the curb stone, fatal injuries being sustained. I have more than once noticed that in these accidents the rider has been mounted upon some favorite horse in which he placed the greatest confidence.

A former schoolmate of mine who was afterwards cadet at West Point, received a severe injury to the left testicle while exercising at jumping.

The only case I ever attended resulting from the kick of a horse, was where a dismounted soldier struck the horse on the flank, the animal promptly kicking him in return. The blow struck the man directly over the symphisis pubis; the bladder was ruptured. We were on the march, and although we gave the man every attention possible, he died within twenty-four hours, in the ambulance, before we could reach a military post.

I have received one kick in the region of the knee from a horse, and that I escaped serious injury was quite remarkable.

A very interesting letter has been received from Edward S. Martin, Esq., who recently contributed a valuable paper on "Hunting" to *Scribner's Magazine*. He states that—"There have been a great number of accidents among the huntsmen of the Genesee Valley Hunt Club, of which none have been fatal, and very few serious. Legs have been broken sometimes, collar bones oftener. The most troublesome case I remember was an accident to the head which occurred three years ago (in 1892), and made serious trouble."

In a letter from Dr. J. H. Brinton, of Philadelphia, he states:

"In the war of 1861-65, I recall several luxations of patella (knee bone), incomplete, or axial, caused by careless and awkward riding, men passing in opposite directions."

From Charles E. Mather, Esq., secretary of the Radner Hunt Club, Bryn Mawr, Pa., I received a report that he knew of no fatal accidents from equestrianism, and of no serious injury to the head, only slight wounds. In one case, the man was laid up for ten days. So far as fractures of the bones are concerned, he states that he has known of but one case in ten years, and that was in the arm. He reports no dislocations and states that in his experience "serious results seldom follow equestrian accidents, unless the horse falls on or rolls on the man, and we have not had such a case."

He has been master of the Radner Hounds for ten years, hunting three times a week during the hunting season, and the field has averaged twenty-five gentlemen and ladies.

The following letter from Surgeon Major Alfred Codd, of the Royal Canadian Dragoons, accompanied his report:

Cavalry Barracks, Winnipeg, Manitoba, Jan. 26, 1896.

Dear Doctor:—I have much pleasure in filling up the questions you desire as far as I am able.

This cavalry force here is now about ten years in existence, and of one hundred strong.

The fractures of the shoulder in both cases were the fracture of the coracoid process, by being thrown off the horse and falling forwards on the joint. The dislocation referred to was that of the elbow joint, by being thrown from his horse and the animal falling on him. The two discharged as unfit for further service, was one kicked by the horse in the groin and severely rupturing the man: the other the man was very forcibly thrown onto the pommel of the saddle and injuring one testicle, causing severe inflammation; the testicle sloughed out completely. The horses in use are all bronchos, and it may be, I think, surprising that I have not to report a larger percentage for the period I mention.

I may say I had one man badly injured on the face by the horse kicking him on the lower jaw, fracturing it, and a dislocation on the other side. Six teeth were knocked out as well. He made a rapid and good recovery."

The following is his report:

Injuries to the head	1%
Fractures of the bones, shoulders, arm and leg	2%
Dislocations	1%
Accidents rendering soldier unfit for service	2%

An interesting letter has been received from H. P. Colt, Jr., secretary of the Genesee Hunt Club:

"I am in receipt of yours of December 17, but can furnish no very accurate knowledge. We have never had a fatal accident in our hunt. The per cent. of accidents from falls in the hunting field, I think very small, as the number of falls is large.

"I think injuries to the head and sprains much more frequent than breaks; and of these, collar bones and ribs more so than limbs. I have never known but one dislocated shoulder, and no broken arms. Our field numbers anywhere from ten to fifty mounts; and in the last ten years I have known of only five broken legs.

"Strains and even breaks of muscles of the inside of the leg occur oftener, and are often very serious.

"I have known of one broken nose. I am sorry I have no more accurate information to give you."

Surgeon George M. Warren, of the Second Canadian Dragoons, states that the most frequent accident coming under his observation, in time of peace, is from the kicks of horses, "striking the men when mounted, anywhere between the foot and knee." Surgeon Warren reports two fatal equestrian accidents, and five attended by injury to the head; twenty where fracture of the bones resulted, and five dislocations. In three cases the soldiers were rendered unfit for further service.

From Surgeon-General Kittredge, of Massachusetts, who is an experienced horseman as well as an able surgeon, comes the report of but one fatal accident from equestrianism. Twentyfive per cent. of the accidents in his experience have resulted in injury to the head. The largest (50 per cent.) has been in fractures of the arm, leg and shoulder. Two per cent. of all have resulted in dislocations of the shoulder. In his military experience not one of these accidents has rendered the soldier totally unfit for further service.

Dr. J. N. Hall, of Colorado, whose valuable paper on "Accidents from Equestrianism Among Cowboys," I have already referred to, in 66 cases not one was fatal; but 13 per cent. of the accidents were attended by injury to the head; 38 per cent. were fractures, 6 per cent. dislocations, and 4 per cent. were dislocations and fractures together. In all these accidents, 2 only were rendered unfit for further service.

The following letter from the British War Office shows that records concerning accidents from equestrianism are not specially tabulated.

War Office, January 29, 1896.

Sir:—With reference to your letter of 17th December last, wherein a request is made for certain statistics in connection with accidents from equestrianism in the cavalry service, I am desired by the Director-General to acquaint you that he regrets that the information required under the several headings does not exist in an available form, in the records of this office.

I have the honor to be, sir,

Your obedient servant,

W. S. Pratt, Surgeon Lieut. Colonel.

From the Surgeon-General of the German Army the following is received:

Berlin, den 22 Januar, 1896.

Sehr geehrter Herr:—Euer Hochwohlgeboren erwidere ich auf das gefällige Schreiben vom 17 December v. J. ganz ergebens, das ich zu meinem aufrichtigsten Bedauern Ihnen Unterlagen für eine statistische

Zusammenstellung der bei der Kavallerie vorkommenden Unglücksfälle und ihrer Folgen nicht zur Verfügung stellen kann.

von Loler, Generalstabsarzt der Armee
und Chef des Sanitätskorps.

From the Swedish Army the following:

Christiania, Feb. 20, 1896.

Dear Sir:—In possession of your favor of December 17, 1895, I regret to say that I am not able to give you the information you require, as our statistics are not arranged in a way that renders it possible to make such extracts as you wish for, and besides, in consequence of the smallness of our exercises and of our standing army, our "material" is so inconsiderable that a statistical report in the way you ask for would probably be of little use.

Respectfully yours,
Fr. Thanlon, Surgeon-General.

I am particularly indebted to the Doctor in Chief of the Federal Army of Switzerland. His letter is as follows:

Berne, January 21, 1896.

Dear Doctor:—Concerning the request made in your circular of December 17, 1895, I can furnish you only with the statistics for the year 1895.

There have been 4,900 calvarymen in service during 23 days (average), which would be equivalent to 317 riders during 360 days.

Of this number, 71 were victims to accidents more or less serious; in no case has the accident proved fatal.

The report is as follows:

Injuries to the head	10
Fractures of the shoulders (clavicle)	8
Fractures of the arm	1
Fractures of the leg	8
Dislocations	14
Contusions (with or without opening)	40

2.8 per cent. of the injured must have recovered.

Accept, Doctor, the assurance of my high esteem,

Physician in Chief of the Federal Army,
Opitz.

From the American Army, I enclose a letter received from the Surgeon-General's office, with statistical tables.

War Department, Surgeon-General's Office,
Washington, January 16, 1896.

Dr. W. Thornton Parker, Groveland, Massachusetts.

Sir:—Your printed circular letter of Dec. 17, 1895, addressed to the Quartermaster-General, U. S. Army, and by him referred to the Surgeon-General, has been turned over to me as in charge of the Statistical Division of this office.

To reply to each of your questions would involve an amount of clerical labor which the office cannot spare from current business. I enclose, however, a consolidation of the statistics of the past five years, giving the average annual ratios, per 1,000 men, of certain specified injuries in the cavalry as compared with similar ratios for the army as a whole during the same period.

By direction of the Surgeon-General,

Very respectfully,

C. G. Smart, Major and Surgeon, U. S. Army.

Injuries, 1890 to 1894 inclusive. (Average annual ratios per 1,000 of mean strength.) (Injuries to "head" not tabulated separately.)

Average annual strength—

	Cavalry.	Army.
Officers	297	1,478
Enlisted men	5,666	22,996

	Contusions and Sprains.		Dislocations		Fractures not Gunshot.		Contused, Incised, Lacerated and Punctured Wounds.		Gunshot Wounds.		All Injuries.	
	Cav.	Army	Cav.	Army	Cav.	Army	Cav.	Army	Cav.	Army	Cav.	Army
Admissions—												
Officers	60.69	39.11	2.02	.81	8.77	5.28	8.77	7.85	1.35	.81	110.59	72.53
Enlisted men	209.04	137.05	3.64	2.44	11.12	7.03	72.94	56.03	6.46	3.71	376.71	262.78
Non-effective—												
Officers	4.04	2.27	.13	.07	1.94	1.24	.27	.36	.55	.32	8.83	5.44
Enlisted men	4.93	3.25	.36	.18	2.12	1.19	2.06	1.58	1.22	.61	13.06	8.56
Discharges—												
Enlisted men	.16	.19	.33	.13	.85	.55	.13	.22	1.17	.69	4.62	3.38
Deaths—												
Officers	.....	.....	.....	.....	.....	.....	.....	.....	.47	.57	1.88	1.23
Enlisted men	.03	.02	.....	.....	.26	.16	.10	.05	2.47	1.29	3.81	2.63

As I have said before, it is impossible to obtain reliable statistics concerning these accidents, since they are not considered of sufficient importance to use the time to tabulate them.

Only in cases where I have found medical men interested in riding—and such cases are comparatively few, for, as a class, physicians do not seem to take to equestrianism—have I received satisfactory replies.

Among southern medical gentlemen before the war, horseback riding was generally in vogue, and is to some extent to-day in our western States.

The more I have inquired into the subject, the more important has the investigation seemed to be.

It is astonishing to me that some of our military doctors have expressed themselves as quite indifferent to the matter of equestrian accidents.

One Lieut.-Colonel and Medical Director in replying, states: "My experience has been *very limited*, and I do not feel warranted in giving any opinion."

In my own opinion, it is very important for the medical corps to be able to furnish, when required, statistics as to the reasonable number of available men likely to be able to report for duty at the front. Statistics as to effective fighting men in an army after the loss from deaths, wounds and sickness, are always valuable.

Statistics concerning recruiting are necessary, so that it can be known at the end of three months and six months as to how many men will be likely to be available for duty in any branch of the service.

It seems to me, therefore, that an investigation concerning accidents from equestrianism is one worthy the attention of medical men in general.

Groveland, Mass.

THE THERAPEUTIC APPLICATION OF CHLOROFORM IN LABOR.*

By JOHN N. UPSHUR, M.D., Richmond, Va.,

Professor of Practice of Medicine in the Medical College of Virginia, President of the Medical Academy of Medicine and Surgery, etc.

Since the primeval curse fell upon our race, and pain and anguish have been the invariable and dreaded accompaniment of man's entry into the world, to soothe woman's sorrows and conduct her safely through the crisis is an object worthy our highest effort, and one to be sought for with commendable devotion.

To accomplish this end, the administration of chloroform has become routine practice, and the concensus of opinion, from a very large number of obstetricians, is in favor of its safety, when thus exhibited. Careful observation for many years has tended to make me question its utility in many cases, nay, to convince me that oftentimes it adds to the peril and prolongs the trial.

That loyal devotion to true obstetric science demands that chloroform in labor should be exhibited just as other therapeutic agents in the treatment of other maladies. We forget that delivery is not a pathological process, but a physiological function; that it becomes pathological only when conditions arise which convert a eutocia into dystocia, or when the physical conformation of the pelvis is such as to prevent the accomplishment of natural delivery by mechanical obstruction, from errors in the pelvic conformation, malposition of the child, or monstrosity. When labor has become pathological in character, and operative interference, manual or instrumental, is demanded, with the dangers of exhaustion from prolonged agony or the addition of surgical shock, the case passes to the domain of the surgeon, so far as the unquestioned exhibition of chloroform is concerned, just as an anæsthetic is demanded in any surgical procedure as a safeguard to life. Or where the condition of the mother is such that her life is imperiled by the supervention of convulsions from existence of any of the systemic conditions which, predisposing to such complication, become active factors so soon as the stimulus of pain, or the concentration of poison to the nervous system sets in motion the morbid phenomena, which manifest themselves by an outward explosion, jeopardizing the life of the infant and of the mother.

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* Read before the Section on Obstetrics of the First Pan-American Medical Congress, September 6, 1893; also Richmond Academy of Medicine and Surgery, March 9, 1897.

It is not with conditions such as these that I purpose dealing, but chloroform therapeutically applied as a causative factor, either predisposing or active, in transforming this physiological function into a condition of some form of dystocia, making aid or interference necessary to the safe delivery of the woman.

The question, therefore, resolves itself into this problem,—in what cases should chloroform be administered; at what stage of labor; what dangers arise in consequence; at what stage do these complications arise; the best means of combatting them; and, finally, these propositions being true, is it justifiable to administer chloroform in natural labor progressing with satisfactory rapidity?

To satisfactorily deal with these queries, we must discuss the physiological action of chloroform so far as it bears upon this question. In the first stage of the administration of chloroform, we have its stimulant effect. The second is the stage of narcosis. In the third stage, the functions of the spinal cord are abolished, as are those of the brain—this the surgical stage; and in the fourth, we have the condition of complete paralysis—death usually by overpowering the respiratory center first. The modes of death are from inflex irritation of the cardiac ganglia; from epileptiform syncope in the stage of stimulation; from paralysis of respiration; from paralysis of heart; and finally from depression by the chloroform narcosis and shock. Chloroform diminishes the excitability of the muscular system and its capacity for work. If prolonged, it exhausts muscular irritability. It interferes with oxidation of the blood, and thus becomes toxic to the fœtus. Its stimulant action on the vaso-motor center is doubtful.

Now, as a practical deduction from these facts as to the action of chloroform, let us apply them to a solution of the queries as enunciated before. As to suitable cases for the administration, we need not consume time in discussing; every skilful and experienced obstetrician will point to those cases in which the pains are nagging and exhausting—in rigid os, in great nervousness and restlessess. I think, too, that except, as just stated, it should not be administered until the second stage of labor is well established, and at the latter part of that stage, and withdrawn so soon as the occiput has passed the ostium vaginæ.

The most serious question is as to the danger arising from the exhibition of chloroform in labor, and it is with this question I would deal with greatest emphasis. Note the danger from reflex irritability, with incomplete anæsthesia; it is at least worthy of investigation that if fatal influence from chloroform does not manifest itself here, still its effect on the heart, through the cardiac ganglia, may be potent enough to, at least, predispose to hæmorrhage, or to weakened heart action during convalescence, with the condition of debility consequent upon it. But the obstetric stage is the beginning of narcosis; it is difficult to keep the patient at this point. Lack of appreciation of what is going on, interferes with voluntary assistance. One of its physiological effects is to diminish muscular excitability, to interfere with muscular capacity for work. Pains are consequently less potent and effective in expelling the child; there is much greater muscular tire, and the natural consequence is that when the second stage is completed, danger of hæmorrhage is much greater from incomplete condensation of the uterus, and I have often seen it occur. The uterus is left in such condition as to make subinvolution, with all the ills that follow in its train, almost inevitable.

The spongy condition of the womb makes the patient much more liable to septic infection. Not only so, but often have I seen labor suspended; nor on the withdrawal of the chloroform, did the uterine pains return with desirable efficiency; evidently its effect on the uterine muscle was toxic in character, and instrumental delivery became a necessity, the agent having transformed a simple physiological process into a pathological one,—thus adding to the peril of the mother. The interference with oxidation of the blood endangers the life of the child, and I make bold to assert my belief that its use in natural labor increases the percentage of still-births. Nor are these the most serious risks. Though few deaths have been reported from chloroform in obstetric practice, it is a question if death supervening in many cases within forty-eight hours after delivery, and reported as heart-clot, etc., may not have been due to the depression from long-continued administration of chloroform, plus the shock of labor. In the cases where uterine contraction is not interfered with, and the woman holds her breath, fixing the chest-wall to more efficiently bear down, she is put in a most favorable condition for the occurrence of epileptiform syncope, when chloroform is being administered; and the retention of carbonized blood in the brain may under such conditions overwhelm the nervous system. The dangers from chloroform exhibitions are most likely to manifest themselves, however, at the close of the second, or during the third stage of labor, and here, post-partum, or accidental hæmorrhage, is the most important, and may be the focus from which may radiate other serious evils.

I would lay down the proposition as an axiom, that whenever chloroform has been administered, a full dose of ergot should be given as soon as the head is delivered or the second stage of labor completed. I have never regretted giving the ergot, and remember no case in which it was omitted that I did not repent of the delinquency. I would also suggest that it is safe practice to give a full dose of quinine (gr. x) at the beginning of the second stage of labor when chloroform is to be exhibited, or belladonna, or one or more doses of nitro-glycerine; especially will this last tend to antidote the nausea following choroform administration.

The hypodermic injection of atropine (gr $\frac{1}{120}$), or sulph. strychnia (gr. $\frac{1}{60}$), will undoubtedly add to the safety of the patient.

We must not lose sight of the fact that in obstetric use of chloroform (which is usually conceded as the safest field for the use of choroform), the patient is much longer under the influence of the anæsthetic, and that the stimulous of constantly recurring uterine contractions, of a painful character, which are supposed to antidote the ill effects of the chloroform by constantly arousing the patient, may be absent, or fail to exercise the stimulant effect usually a consequence of pain.

In the light of the foregoing facts, I most earnestly avow my belief that we, as physicians, should place chloroform upon the same platform as other drugs; not to be influenced by our sympathies aroused by the pleadings of patients, or the fashionable routine practice of the day, but initiate and sustain a much needed reform in our obstetric work, chloroform being administered, as other agents, when the indications in the case imperatively demand it—not unless. He is a bold man who, invading the domain of nature, interferes with her physiological processes, and places the wife and mother in a position of increased peril, and perchance, the shadow of a fatal issue, or, at least, a life of invalidism and suffering, where before the home was irradiated with the effulgent rays of the sunlight of true and unalloyed happiness.

210 West Grace Street.

TRANSLATION.

PARALLEL BETWEEN TRACHEOTOMY AND INTUBATION IN CROUP.

By Dr. SEVESTE.
Translated for Gaillard's Medical Journal,
By H. McS. GAMBLE, Ph D., Moorefield, West Va.

From the period when it was, for the first time, employed successfully by Bretonneau, then popularized and placed in honor by Trousseau, tracheotomy has remained, until these latter years, the only means at our command for combatting laryngeal asphyxia in croup.

In 1858, however, Bouchut proposed, under the name of *tubage de la glotte,* another method, consisting in the introduction by way of the mouth of a tube into the larynx and in thus re-establishing the permeability of the air passages. In a paper presented to the Academy of Medicine, Bouchut reported two cases which appeared to him to establish: (1) The facility we possess of practicing intubation of the glottis by means of a tube or canula fixed by the inferior vocal cords and not preventing the functions of the epiglottis; (2) the tolerance of this tube (virole) by the larynx; (3) the possibility of relieving the asphyxia of croup and of diseases of the larynx by this means, in preference to tracheotomy; (4) the facility that the large pseudo-membraneous concretions, formed in the trachea and in the bronchi have for escaping by this intra-glottic tube; (5) the utility of this new resource to physicians who, in small communities, without assistants and far from all help, will be able to employ this means in preference to all others.

The new method was violently attacked by Trousseau: the apostle of tracheotomy could not, in effect, bear that one should seek to restrict the importance of an operation which he had, not without trouble, succeeded in establishing in the treatment of croup; he brought into the contest all the resources of his powerful logic and, thanks to his uncontested authority, he succeeded, in spite of the opposition of Malgaigne, in inducing the Academy to adopt the following conclusions: (1) Intubation of the larynx as it has been applied up to the present time, has not appeared to us either sufficiently useful, or sufficiently free from danger to merit the approbation of the Academy; (2) tracheotomy, in the present state of science, is the only means to be employed when there no longer remain any other chances of safety in the employment of medical measures.

These conclusions, following a widely known discussion, were considered by everybody as the formal condemnation of intubation. It is to be observed, however, that they contain certain reservations, as if they had been afraid to pledge the future, and it is proper to add, to the utterance of the Academy, that the instrumentation of Bouchut was far from being perfect, that the observations reported by him were neither sufficiently numerous nor sufficiently conclusive to carry conviction, and that, in short, the method was not sufficiently matured.

In the course of this discussion, Malgaigne uttered these words: "Let the history of the lithotrity serve us as an example! Who knows if 'tubage' will not one day be to croup what lithotrity is to stones in the bladder?"

The event was to prove his view correct later on. In 1881, a laryngologist of New York, O'Dwyer, conceived the same idea as Bouchut, and without knowing, it appears, about the works of the French physician, invented in his turn, or rather re-invented tubage under the name of intubation of the larynx; but, more discreet or more patient than Bouchut, he was in no hurry to publish his method; he began to study it ardently, sought for the best instrumental combinations, perfected the outfit of tools and brought it to such a degree of perfection that to this day the whole world has found it convenient to make use of it without subjecting it to any important modifications. It was then only, and when he had collected an imposing number of favorable results, that O'Dwyer made known the principles and the application of the new method. If the discovery of intubation belongs, beyond the possibility of a doubt, to Bouchut, it must be admitted that the dissemination of the knowledge of this mode of intervention is due to O'Dwyer; the latter has been even more than one who makes common the knowledge of a truth, and it is but just to say, as was done at the Congress of Berlin, that intubation (tubage) has two fathers: Bouchut and O'Dwyer.

Intubation was practiced first in America, and was not long there in almost completely supplanting tracheotomy; then it extended into England, into Germany, into Austria, into Russia, but met with but few partisans in France, where a few trials only have been made of it. Jocques (de Marseille) was for a long time the only one who had recourse to intubation in a regular manner. During this time the new method made some progress in Italy, in Spain, everywhere, except in France, until the close of the year 1894.

At this moment, the serumtherapy commenced to be applied by the majority of physicians and, from this fact, commenced also a new era for operative intervention in croup. This interventon was no longer, in effect, a simple expedient employed at all hazard to prevent the patient dying asphyxiated, without giving him any chances of final recovery: it was a means of gaining time, of prolonging the life of the patient until the moment at which the serum might become efficacious and might be able to save him. It was simply a question of gaining two or three days in general, often even only twenty-four hours, and under these circumstances it is self evident that if the result could be obtained without traumatism, the benefit was not to be despised.

It was then that intubation, until that time almost unknown in France, began to be applied on this side and on that. The trials were at first timed, but few; but soon they were repeated and intubation came very quickly to almost entirely supplant tracheotomy. At Paris, for the last two years, in both hospitals for children, intubation has been the rule, tracheotomy the exception.

It seems to us then that the time has arrived to compare the two modes of intervention with one another and to ascertain if one of the two is to be preferred to the other. It is to this particular question that we shall confine the present study, based particularly upon the examination of cases seen within the last two years in the service pertaining to diphtheria, in the Hospital des Enfants-Malades.

It seems *a priori* that, to settle the question, it suffices to compare the results furnished by the different statistics that have been published up to this date; but these statistics, as well for tracheotomy as for intubation, are very discrepant: very satisfactory in some series of cases, they are in others more than mediocre in value. At the very most one may endeavor to draw a conclusion from the sum total of these statistics: this is what M. Gillet has attempted. In going over a very great number of works on this subject, he succeeded in collecting a total of 8,457 intubations which have given 2,571 recoveries, that is to say a proportion of 29.94 per cent.; or in round numbers 30 per cent. For tracheotomy he arrived at a total of 16.293 operations, with 4,902 cases, that is 33.2 per cent. On the other hand another reckoning made by Prescott and Goodtwait shows in a total number of 23,941 tracheotomies a proportion of 28.97 per cent. of cures. In combining these two results, one may consider as very near the truth that the figure of 30 per cent., that is to say a proportion equal to that which intubation yields.

These statistics, in one case as in the other, are all anterior to the employment of the serum in the treatment of diphtheria, and it would be interesting to continue the comparision since the application of the new treatment. Unfortunately this is impossible for us to do; we well know that tubage gives to-day a proportion of 75 per cent. on an average, and often a higher one, but we are left in ignorance as to what tracheotomy would do under similar circumstances. In fact, for the last two years, intubation has been almost exclusively employed in the treatment of laryngeal obstruction and tracheotomy has only been done in very rare cases, nearly always in desperate cases, when intubation could not be performed or had not relieved the patient; there is therefore no comparison to be established between the two series of cases.

It is elsewhere that we must seek for the elements of valuation to enable us to resolve the question: we fortunately find some of them very clear and very characteristic in the comparative examination of the operation itself, and of the accidents that may arise during its course or supervene within the next few days.

Tracheotomy constitutes an operation that is often very difficult, and at the very least one that is extremely rough and futile in unexpected incidents; if an operator who is able and instructed by a frequent performance of tracheotomy succeeds in general in triumphing over these surprises, they may confuse or even completely bewilder a physician unfamiliar with the operation and who besides will have often passed several months or even several years without having performed it. If he has not taken his points de repère with an absolute precision, if he deviates ever so little from the median line, he is exposed to the risk of committing an operative error which will often be difficult to repair and which may even entirely compromise the final result. There will be the same risks again, if the assistant charged with the duty of fixing the head of the patient does not keep it absolutely immobile and straight, in a state of sufficient extension, but not an exaggerated one. The operation may be likewise complicated by a hæmorrhage, by a syncope, by an attack of asphyxia or even a complete apnœa. Even when the operation has been terminated by the introduction of the canula into the trachea, the patient may still remain some time in a state of apparent death, from which one does not always succeed in rescuing him. To sum up all, the most skilful, the most experienced operator, is never absolutely certain of bringing things to a successful issue. Archamboult, who had a very great experience in tracheotomy, and who had performed it with as much safety as elegance, always took care to declare to the family that he could not be sure that the child would not remain dead upon his hands.

For intubation, it is no longer altogether the same thing; assuredly, there is likewise involved a delicate operation, demanding a great precision; but if one has been careful to educate the hand by some preparatory exercises, if one proceeds with gentleness and with prudence, there is but very little danger of grave accidents to be apprehended.

Two conditions are indispensable for the successful performance of intubation: the index of the left hand should possess an exquisite delicacy of touch, in order to be able to quickly find the epiglottis and the arytenoid cartilages serving as points de repère; besides, the right hand, which, furnished with the introductor, will conduct the tube into the larynx, ought to be trained to manipulate this instrument without stiffness, but with a peculiar suppleness of the wrist and at the same time with a firmness which assures a precision of the movements. The education of the left index is also quickly accomplished, if one practices placing the finger upon the base of the tongue and in making examinations below the epiglottis and the superior orifice of the larynx; this exploration can be made without the least danger, upon healthy children of different ages, in the course of examinations pertaining to the throat.

For the right hand, it is important to accustom oneself to the manipulation of the instrument at first in holding it in the hand and communicating to it the proper movements while one has it under the eyes; one finishes then by a few exercises upon a fantom furnished with a rubber larynx and finally upon the cadaver.

When, by these different means, one has sufficiently trained the left index and the right hand, one may risk practicing intubation upon the living without being exposed to accidents capable of compromising the result. Of these accidents the one most frequently observed, especially with inexperienced operators, consists in the penetration of the tube into the œsophagus; it is readily noticed by observing that the air does not pass through the tube and that the child is not relieved; one also remarks, in general, that the thread attached to the tube disappears, so to speak, visibly, drawn as it is by the weight of the tube which descends into the œsophagus; at that moment, it suffices to withdraw the tube and to commence the operation anew.

False passages through the larynx and hæmorrhages are extremely rare and can result only from rough and awkward maneuvres; the acts of vomiting provoked by the contact of the tube do not require one to stop; as to the attacks of syncope, they may be produced during the introduction of the tube as well as in tracheotomy, but are imputable to the state of the child at the moment of the operation much more than to the intubation itself.

On the contrary, the increase of the dyspnœa during the intubation or in the following moment results generally from obstruction of the tube by a false membrane pushed back or detached; this is a serious accident, but one that can be ordinarily avoided by withdrawing the tube at once, and then it happens, in a given number of cases, that the child rejects in a paroxysm of coughing, the false membrane that caused the obstruction; afterwards, the relief is often so great that we can dispense with replacing the tube, at least for a certain length of time. In the opposite case, and if a new attempt to introduce the tube was not attended with success, one would be free to perform tracheotomy; this is, however, a circumstance that rarely occurs.

Upon the whole, the operation of introducing the tube is, without contradiction, more easy to perform than tracheotomy and is much less exposed to accidents of a serious character or impossible to forsee. It presents also some advantages from other ponts of view: the wound of a tracheotomy constitutes, in effect, a surface of absorption by favor of which secondary infections may be produced, and even, without taking account of the local complications of the wound, it is undoubtedly true that broncho-pneumonia is more frequent after tracheotomy than it is subsequent to the introduction of the tube.

Finally, when because of these pulmonary lesions or from any other cause, fever exists, the application of cold baths is almost impossible in the case of a child whose trachea is open and whose neck and upper portion of the chest cannot be immersed, whilst after intubation balmotherapy is as easy to practice as in a child not intubated.

However, whatever may be the superiority of intubation in principle over tracheotomy, it must be admitted that there is one dark spot; this is a fact that cannot be insisted upon too much. After tracheotomy the child may be abandoned to the care of the mother or of a nurse of very little intelligence; there is seldom more to do than to clean the internal canula if it becomes obstructed; the prescriptions relative to alimentation, to æration, etc., are very easy of execution. On the contrary, after intubation, the child should be the object of an altogether special surveillance; this is not only because deglutition may be difficult and that certain measures must be adopted in order to facilitate it, it is above all by reason of other complications much more serious and much more important.

The tube may be rejected, and the occurrence is not altogether rare; but, if sometimes, after this spontaneous rejection, accompanied pretty often by expulsion of false membranes, the child breathes comfortably and is able to dispense with his tube, it more frequently happens, above all within the first two days, that the intubation must be performed anew at the very moment, or in a time more or less near it.

On the other hand, the tube may be found to have become obstructed, it may be all at once, it may be in a slow and progressive manner.

The false membranes existing in the trochea or in the bronchial tubes and detached particularly under the influence of the serum in general very easily traverse the caliber of the tube, in spite of the small diameter of the latter; but, in some cases, they gather into clusters below the inferior orifice and suddenly determine a more or less complete obstruction: it is then necessary to withdraw the tube immediately; if this was all that was involved, one might rigorously teach the person who watches over the patient the manner of doing it, but in a goodly number of cases it is necessary again to retubate the child and, for that, the presence of the physician is indispensable.

When the obstruction of the tube takes place slowly by the successive deposition upon its internal surface of dried mucosities of the debris of false membranes, the danger is less pressing, but none the less it exists.

In conclusion, an intubated child ought to remain under the direct and immediate surveillance of the physician who has performed the intubation or of some other accustomed to the operation. This is possible in a hospital, where the interne on duty can always arrive in the space of a few minutes; still it is necessary for this that the apartment for attendants be not too remote from the diphtheria service and, besides, that the personnel of the service be experienced in performing the withdrawal of the tube upon the first alarm of sudden obstruction. But, in the city practice and with still greater reason in that of the country, if the physician cannot tie himself down to remain near the patient during two or three days at least, he ought at once to abandon intubation and decide upon tracheotomy.

Tracheotomy may also, under some circumstances that, however, are quite rare, respond to certain special indications. It happens sometimes that, it may be from the presence of a very intense laryngeal spasm, it may be for any other reason, the tube cannot be introduced into the larynx; the attempt may be repeated, but after two or three failures there is no use in persisting, and, if the contraction continues in a threatening manner, one must resort to tracheotomy without waiting until the child is exhausted. The same is true in those cases in which the child has not been relieved by the introduction of the tube; this may result from the presence of false membranes in the lower part of the trachea or in the bronchi; it is to be feared that in such a case tracheotomy will be no more successful, but it ought at least to be tried.

Tracheotomy is likewise indicated when one has reason to suppose, it may be before any intervention, it may be after a first intubation, that the trachea contains abundant mucosities or voluminous false membranes, which might greatly endanger the determination of an obstruction of the tube.

Certain children reject the tube sometimes in a manner in some sort undefined, even when one has employed a tube of a caliber greater than that which corresponds to their age; if one finds himself thus exposed to the necessity of introducing the tube too often, it may be preferable to have recourse to tracheotomy.

Finally, the same will be true likewise in certain cases in which, after a prolonged sojourn in the larynx, the tube cannot be removed under penalty of asphyxia, either from the presence of a persistent spasm, or because of a sub-laryngeal constriction.

Save these particular cases, and as a general conclusion of this work, we will say: intubation is without possible dispute, the method of choice in operative intervention of croup; but it ought not to be practiced unless the patient can remain under the direct and immediate surveillance of a physician accustomed to the operation; outside of these conditions, it is to tracheotomy that it is necessary to have recourse.

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SELECTIONS.

LECTURE.

MEDICINE AS A PROFESSION.[1]

By David W. Cheever, M.D., LL.D.

Medicine as a profession brings the doctor into contact with all kinds of men, women and children—of all ages, from birth, literally, to death. No other relation of life, except the family relation, is so close; and in some respects, it even surpasses that. The peculiar relation of the patient to the doctor is different from any other. It makes no difference to the doctor, when he has a case, what sort of a coat happens to cover it, or of what condition in life the patient may be. If he has a true professional spirit he sees only the sick person, and he keeps before him the only idea which can properly belong to the medical profession—that his business is, first, to try to relieve suffering, and cure sickness; and secondly, if he cannot fully accomplish those things, at any rate to prolong life and render the close of life and the approach of death more easy. In that way, in some respects, his calling is, I think, even more sacred than that of the clergyman; because it deals with tangible and known things that we all can realize; so the good that he does is soon apparent; the mistakes that he makes are soon plain to be seen; and the reproaches that he may make himself for those mistakes are sometimes very bitter when he realizes the truth. Thus thrown into such very intimate relations with humanity, he becomes a participator in all its secrets; and he frequently knows all the secrets of the family, and even the secrets of certain portions of the family that are not known to the rest. He is, so to speak, a father confessor to all classes of human ills and sicknesses, and, of course, should hold it a point of honor to keep these secrets. It used to be the rule that a physician should take oath on beginning to practise his profession that no secret imparted to him in the sick-room should ever leave it.[2] This rule is as important to-day as it ever was, perhaps more so; because in these days of modern publicity, when there is no private life, who shall be the guardian of these important interests, if not the physician and the clergyman—the two forms of confessor to whom human beings go in their misery to tell their sins, their sorrows or their physical infirmities. What, then, shall we think of the law, which, in the State of Massachusetts, does not protect the physician in court in the keeping of secrets, but obliges him to tell them in certain circumstances, under penalty of imprisonment? It seems to me that this is a shame to Massachusetts, because it is in this State and some others, but not in all the States in the Union, that this condition of the law prevails. While here, as I am told by those who know the law, and as I have experienced in court, the physician is not protected in refusing to divulge the secrets of his patients. He may absent himself. He may, if he thinks proper, tell lies; but that is perjury. Certainly this is not the way in which the law should be respected and obeyed; for where the people make the laws, they should be amended, but not violated. A number of the States of these United States protect the doctor in the possession and retention of family secrets. Some States go farther, and being more advanced in humane legislation than we are, make it criminal for the doctor to reveal the secrets of his patient in court. He is not only protected in his effort to keep them, but he is forbidden in the State of New York, to divulge them; and also in seven other States. In Massachusetts, the law still reads, as I understand it, that the physician has no defense; that if the counsel or judge think it is for the best interests of the case that certain statements should be made, they must be made. No one would deny that this ought to be done in serious criminal procedures which involve life or death; but that in ordinary civil suits affecing only the reputation of the individual who, perhaps, is on trial or is forced on trial, private affairs should thus be dragged into publicity, seems to me extremely wrong. The doctor has no redress unless he refuses and is punished. I hope that the profession here will soon take steps to have this legislation changed.[3]

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[1] A Lecture given in the Post-graduate Course at the Harvard Medical School, October 22, 1896.

[2] The oath of Hippocrates also forbade the taking of fœtal life. It is the doctor's business to save life, not to destroy it. No occasion should tempt the physician to destroy an infant except to save the mother. Whether, under proper restrictions, he might be allowed to hasten the exit of the really moribund sufferer, and promote euthanasy, is a question.

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[3] *Greenleaf on Evidence* (Edition of 1896, vol. i, p. 248, under "Privileged Communications").—"Neither is protection extended to medical persons in regard to information they have acquired confidentially, by attending in their professional characters."

Protection by common law is not extended to priests; although the Roman Church regards the confession as made to God, and forbids its revelation; and in the English Church, the clergyman telling is "under pain of irregularity." Anything told to legal counsel, however, is a privileged communication, and cannot be questioned, unless the party making it waives his rights as to secrecy. Massachusetts courts go by the common law, and oblige the physician to tell the secret confidences of his patient, and to lay bare his diseases or his sins. Not so New York, Missouri, Indiana, Nebraska, California, Wisconsin, Michigan, Minnesota—other States I have no knowledge of.

Revised Statutes of New York (vol. ii, p. 406, paragraphs 72, 73).—"No person duly authorized to practise medicine or surgery shall be allowed to disclose information acquired in attending a patient in a professional character, and which information was necessary to enable him to prescribe for him, or to do any act as a surgeon."

The New York statute has been held to apply also to knowledge acquired by observations of the patient's symptoms, or even the statements of others present. Paragraph 72 extends the same privilege to priests and ministers.

The doctor is a victim of the interviewer and reporter: they not only pursue the doctor, but haunt the house of the sick, and even the chamber of death. There is no privacy, no respect for the patient's feelings. If any person, in a public position, happens to be sick or dying, the papers want it all, and without regard to the feelings of the friends. Now, what course must the doctor take when pursued in this way? Reporters come from his own town, or from distant cities, and claim that the illness of this person, and whether he is to live or die, is of great importance to the public. Why? Because it may affect some political deal; because it may affect the stock market; certain changes may take place if he is about to die; and so on. To whom is this man's reputation and life so important as to his own family? It seems to me the only answer for a physician to make, is to say: "If you will bring me a note from the nearest relatives requesting me to give you such knowledge as I can of this patient, you shall have it; otherwise I shall say nothing; and you may place whatever interpretation you please on my silence."

There is another situation into which we are thrown, where the preservation of secrecy is doubly important, because the patient is beside himself and says things that he would not say if in perfect control of his senses. I allude to that condition where the patient is under the influence of anæsthetics, especially ether. It is an intoxicant; it is a stimulant; it incites people to talk; they do not know what they are saying; everything is exaggerated in their mind; they usually feel that they are being abused; and they frequently use very strong language, which they would not do under ordinary conditions. So also, in coming out of ether, many men, but especially women, pass into a state of hysterical excitement where everything becomes exaggerated; and they frequently say things which they would rather not have said, and sometimes betray secrets which they would rather not have spoken of. Now here is a position in which the doctor, the nurse, and all the attendants are placed under a double bond of secrecy; here is a helpless individual whom you have reduced to a condition in which he knows nothing, and it is worse than indiscretion on the doctor's part to divulge anything that transpires while the patient is in such condition. He should consider that anything said there has not been said; that anything that has been heard there has never been heard; and that no reference should ever be made to it. If one begins to tell such things the habit grows upon him; consequently here is where silence illustrates the rule, that speech is silver and silence is gold. It is doubly so in the sick-room, where an anæsthetic has been used.

Now, we know a great deal more about their condition than the patients themselves. Shall we always tell them the truth, or not? They wish, apparently, to know the truth, and yet they dread to know it. I mean this, of course, of quite sick people. Sometimes a patient will come into your office and say: "I have been asked to come and see you because I have a tumor here; I don't care about it; it does not make the slightest difference to me what it is; nothing would affect me. I want the truth, and you may be sure that it would not trouble me at all." Now, that is all false—all false; and you must so consider it. They do not think so; for anything you tell them will have a deadly effect. No man can bear the truth; it takes away the chances of his recovery. If he is about to have an operation, if some growth is to be removed from him, and if he learns that what is removed from him is but temporary relief from another operation which must follow, this will prevent rapid convalescence, and will weigh upon him all the rest of his life. And the surgeon may be mistaken; it may not come back so quickly. Meantime you have pressed him down beyond the hope of recovery. How much worse must this be in the case of acute sickness, as in a prolonged case of consumption, where the patient having lingered along for a good while finally asks the physician what he thinks. You know he is going to die; you know sometimes it will take place in a few days or hours. Are you going to tell him? Here comes one of the hardest moments of the doctor's life. He should consider it his duty to tell the friends, but whether he should tell the patient must be left to the doctor's judgment. There are some cases where he had better lie, I think, although the patient may live only a few days. There are other cases, of course, where he should hesitate to leave him in doubt, on account of the fact that there can be no recovery, and the patient should know if he wants to; again, he should not prevaricate too much, out of regard for his own position, as he has to protect his own reputation; and if he gives an utterly false prognostication, and a day or two shows it false, it injures him and is the subject of a great deal of talk. But it is not right to take away from a patient any hope which may aid in his recovery.

There are cases when friends come to the doctor and say, "You know that this is going to terminate fatally?" "Yes." "You know it will be pretty soon; do you realize that unless the patient knows it and arranges his affairs, it is going to leave us beggars?" Now that is not an uncommon case, and if you can assure yourself that it is true, of course you feel obliged to speak to the patient; but if you can, try to make light of it; telling him that he should arrange his affairs, as we should all arrange our affairs and so on; yet anything you say, at any time, will be quickly picked up by the patient, and, perhaps, be thought to mean more than you do mean. Here, also, is another reason why doctors should be cautious and reticent; because their stories are doubled the second time they are told, and tripled the third. Nothing that a doctor says retains its simplicity; when passed around from mouth to mouth, it is added to, and speedily grows into something entirely different from what he would have wished to utter.

I need not say, of course, that, speaking in a selfish point of view, it is extremely important that in the ordinary course of life the doctor should be reticent among his patients, because if he gets the reputation of giving away their private affairs, it soon injures him in the community. One who can be entirely reticent wins more respect, and retains more the confidence of all the world. So much for the doctor's immediate relations to the patient as regards his secrets, and as regards the necessity of telling him truth when he is sick.

Now, there is this peculiarity in the relation of doctor and patient, that if a doctor once enters upon a case, he does not feel that he can give it up voluntarily until the patient sees fit to discharge him; as long as the patient wishes him to go, he must go, with or without prospect of reward. If he has not touched the case, all right;[4] but if he has once taken it up, he must go on. How often this happens with a young doctor whose first patients are among the poorer classes. Their means are soon exhausted, money is gone, no more money is earned, but that patient has got to be seen through, no matter if it takes a month or a year. Of course there are cases where the patient wishes to change, and then the doctor has nothing to do but to retire. The relations of the doctor and patient are peculiar, they are so close and intimate; and they are of a most friendly and charming character when confidence is preserved; but as soon as confidence in the physician has begun to waver, then is the time the doctor is of no use. He had better be excused, and another take his place.

Consultation may be called for various reasons. In the first place it may be called, as it ought to be in most cases, solely for the good of the patient. It may be called, secondly, by the doctor to protect himself; it is wiser for the doctor, I think, if he finds the patient wavering, to suggest a consultation, as this is much better than if the suggestion comes from the patient. He may not want a consultation at all; he may feel sure of his case; he may see it all in his mind; and he may regard any further advice as perfectly idle; but at the same time, if the patient, or, as is more common, if officious friends, insist on having further advice, he had better have it; and then, if he has it—with whom? Any reputable physician with whom he can agree in therapeutics; but when one general practitioner wants another practitioner, what is the use of having a consultation if they believe in such opposite directions that they can't get together to as to prescribe the same medicine?[5]

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[4] The doctor is not obliged to go when called; the only law which binds him is the obligation of humanity. Thus, if in an isolated community and no other physician were to be found, he might feel obliged, while he would not in a city or town where there were other doctors. Fatigue, sickness, overwork, other engagements, are sufficient excuses. Were it otherwise, he would soon die of exhaustion He may decline, day or night; he can select his patients as he chooses; but if he goes, he feels obliged to continue.

Now, doctors have their quarrels, a great many of them; they have very bitter feelings toward each other. One's enemy, as one conceives it, perhaps may be the person selected by the family to be called in consultation. One may expect, if such a person is called, severe criticism may be made, or the confidence of the family undermined; but if all parties would observe that rule laid down in one of the earlier treatises, "No man ought ever to go to a consultation who carries into it any of his private grievances, or who can see in it anything but the case of his patient," then we would have no trouble at all. Now, when does a doctor want a consultation to protect himself? In doubtful injuries; with ill-minded people, contentious people, people who are likely to make trouble, people who are never satisfied with anything that is done for them. The doctor soon estimates that class of people: he sees a great many of them; he can select them and differentiate them in about five minutes after he is called;—and that is the class against which he is very wary, and the class against whom he seeks to protect himself.

Now let us consider one other thing. Don't call the consultant to such a job as this unless you warn him of what he is going to see. He is going to be in it as much as you; if there is to be a lawsuit the consultant can be sued as much as the attendant; and consequently it is not fair to lead him into this without giving him due warning; but no reputable man of good standing will refuse you in going to consult, or in going to help you in court, or to back you up, in any honorable way, in the profession. Doctors stand together very closely. Some one said to me once, that they never saw any Masonry like the Masonry of doctors. Doctors have to protect each other; not to the extent of doing wrong, but to cover up any misfortune of their brethren so far as they can, in order that the general respectability of the profession be preserved. It is right that it should be so.

Now, doctors, as I say, hang together very closely, and there are certain ethical relations that they ought to adopt and preserve to retain each other's confidence and respect. We used to hear a good deal in old times about stealing patients; we don't hear so much about that nowadays, because the idea that any patient belongs to any doctor has long since ceased to exist, on account of the great number of specialists, and patients frequently pass from one doctor to another; and such a thing as a lifelong doctor seldom exists in the larger communities. What happens is this: the patients are variable; they wander about from person to person; and they think, unfortunately, that they have a good deal of medical knowledge which they do not have. They are taught by the newspapers and various forms of pernicious literature, that certain things are to be applied in certain cases, and they undertake to judge for themselves, generally coming to grief in consequence. Now I contend that the person who is constantly changing his doctor is very unhappy, just like a person changing his religion.

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[5] *McClelland on Civil Malpractice* (p. 15).—"Among practitioners of the different schools consultations cannot be held, for the reason that there is a radical difference between them either as to the medicines to be used, or the manner of using them; hence, if the practitioners be honest in their several beliefs, no good can accrue to the patient, this being the sole object."

The class of things that doctors ought not to do towards each other are perhaps rather difficult to define; but they might be described as making remarks of a derogatory character, or innuendos, or insinuations, or inquiries in regard to a case in the hands of another physician, or the suggestion as to whether certain things have been tried. And, also, you should be very chary, I think, in showing too much attention to the patients of other doctors. Sometimes you get yourself considered very ill-natured and ill-bred on this account. Of course, you ought to have a certain reservation about visiting people in a friendly way while they are under the care of other physicians. It might seem at first sight, that this is of no consequence; but when you consider it, it is. The doctor goes to see a friend who is sick and sits down and talks with him; naturally the patient leads the conversation to his own condition; he imagines or makes up something which he supposes the doctor has said, which reaches the ears of the attending physician and perhaps wounds him; consequently the wider you can keep from other people's patients while they are sick, the better for all concerned.

I conceive it to be the duty of the doctor as a professional man, to help to take care of the poor; and for his interests to do dispensary and hospital work whenever he can get an opportunity. In doing this I consider it his duty to teach, if any one comes and wants to be taught; in other words, the functions of dispensaries and hospitals are not simply to take care of the sick, but to spread medical knowledge. These are the great fields where experiment can be made, where aggregations of cases can be collected together, where statistics can be of some value, where patients are held down by stern laws and severe discipline, where their cases can be compared as they cannot be in private houses. While we should bear in mind that the first duty of the hospital is to take in the sick and injured and care for them, and try to make them well, there is another, and perhaps as noble a function—to extend medical knowledge, and also to allow others to derive benefit from it, and to learn, if they wish to come to the hospital and be taught. It seems to me that the physician who takes a hospital position and does nothing but make his visits and never makes it public to others, does not fulfil his entire duty; for those who have any light in this respect certainly ought to give it to others. It is thus only that students can learn; it is thus only that young physicians can learn; and, I venture to say, that you will find in hospitals a good many older physicians, who learn much from these larger fields of sickness.

The doctor, in his profession, loses much of the benefit of his calling and does wrong if he does not visit medical societies and associations. If he holds himself aloof from his fellows he gets to be regarded as a singular person, and he does not get along so well. The same duty that leads him to teach in hospitals should lead him to go to medical gatherings. They are of extreme importance; if possible, as much so as the hospital or dispensary.

To recapitulate, for a moment: We, then, should treat our patients always as our nearest friends; should preserve their secrets so far as the law will let us; should respect their confidence; should tell them the truth, unless it is going to injure them, and then should hesitate about telling them. We should, with our brother physicians, take extreme care in order that no suspicion may arise in their minds that we are suggesting anything in regard to their patients, or doing anything to make them lose their patients. We should never approach a consultation unless we can say fully that we have no feeling except for the best welfare of the patient. We should seek hospital positions because we can learn thereby; but having sought them, we should not hold them unless we are willing to teach, and impart some of that knowledge to others.

The doctor has certain duties which he owes to himself; and laying aside that very selfish mode of looking at the profession that we considered the other evening, still we must concede, even from a professional point of view, that the doctor must be paid in order to get his living, in order to sustain himself at all; and it is a part of his duty and right to collect his bills. Here comes in the great difficulty of the sliding scale of which I spoke in the other lecture, which is absolutely necessary on account of the peculiar status of his patients—some being poor, some middle-class, some rich. No one price can apply to them all. They all may go to the grocer's and pay the same price for a pound of sugar; but they cannot come to the doctor and pay the same price for medical attendance. The poor, being unable to pay for it, would be untreated, to the great injury of humanity. Consequently there must be a sliding scale from zero up; and a good deal of it must be zero. The doctor, in making out his bills and collecting them, must have regard to the circumstances of his patients; be just and merciful; but wherever people have any means, it is due to his self-respect to make some charge; and it is better for the patient as well as the doctor.[6]

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[6] Exorbitant fees injure the standing of the profession. Liberal fees, in consultation, are a protection to the attending physician. Small fees to the poor—but some fee—are the true course for the young doctor.

Questions frequently arise between physicians as to paying brother physicians for the care of themselves and families. It is the custom of doctors not to charge for such services; but it is the custom among some physicians, when a physician is called in, to arrange the matter by sending a present, or something of that kind. Others do not do so.

Now there arise many cases where doctors who are skilled in some special branch are called from a distance to see physician's families, and thereby lose a great deal of time from their regular practice. When a person is a specialist he may be called away a half a day or a whole day from his business. It would be a great hardship if the doctor was obliged to lose so much time without charge; consequently a rule was made that for visits to brother physicians which cost much time, one-half of the usual fee should be expected; and this seems to me very just.

Then, another rule in regard to the fees in visiting at a distance; and that is, that we should not charge by distance, but by time. Distance is not what it used to be: what takes now an hour used to take a day; if you can get an express train and can run over forty miles in an hour and come back in another, what is the difference whether you do that, or drive eight miles out of town for an hour, and come back in another hour? Consequently charges should be based on time rather than on distance, to a reasonable degree.

The doctor, unfortunately, is frequently the victim of suits for malpractice. He cannot afford to compound those felonies. What is it to compound a felony? If a thief steals your watch, you can advertise and offer a reward for its return, with no questions asked. A doctor cannot do that in a threatened law suit. The moment he does that he has lost his self-respect, and the respect of the community. If he is sued for malpractice he has got to fight, and fight it to the bitter end. No suit for malpractice should be condoned or settled in any way, but should be fought out. As a rule, however long it is fought, the doctor will win his case. It is rare in Massachusetts that the jury will go against the doctor, in the long run, unless he proves to be a man of disreputable character. As a rule, the law is lenient, the law is just; and the interpretation put on it by most of our judges is rather on the side of the physician. It reads somewhat in this way: "The doctor is not obliged to be possessed of extraordinary skill; if it can be shown that he had ordinary skill, that he did the best to use the skill which he had, that the skill that he used on his patient was what is averaged by the medical profession in his vicinity; if it can be proved that he did not neglect his patient, that he did the best he could, then he cannot be cast in damages in a suit for malpractice."[7] I have looked over the records very carefully, and the judges are uniformly fair, in their charges, to the doctor. Now, what brings these suits? Frequently surgical treatment, occasionally medical, more rarely obstetrical; but more frequently is this the case in surgery, because surgery is tangible and can be seen. If a man has a bad limb or deformity, all his neighbors see it, and by and by some one says, "Why, that is not so good as mine, that is not as good as it ought to be;" or, worse still, some other doctor inadvertently says, "That is not a very good result." Immediately his mind is disturbed, and he brings suit against the doctor. In the present state of the law, we consider this a great hardship, even greater than being obliged to tell secrets in court. Under the present state of the law the poorest person can go to court to get justice against the doctor without having to possess one cent; he can get some counsel to undertake the case for the sake of the prospective fees, and consequently he can sue the doctor without money and without risk. Meanwhile the doctor cannot afford not to defend it, and to defend it by good counsel; he is very foolish if he has poor counsel. At the end of the suit he comes out triumphantly, but with a loss of several hundred dollars; while the only person on the other side who loses anything is the lawyer, who has taken it up for what he can get.

We are all liable to be sued on the slightest provocation, and we must defend it. We are not the only ones who suffer; if you look at the records for actions of tort, as they are called, you will see that the courts are loaded down with them. Corporations, municipalities, individuals, merchants, business men, contractors and employers, are constantly blackmailed in the most shameful way; many of these cases they settle without allowing them to go into court. So that our profession is not alone or peculiar in this respect; only it seems to us very hard, when we have done the best we could, that we should be for a long while kept in a state of uncertainty and forced to pay in the end, although we get a verdict triumphantly in our favor. Now here comes in sometimes the importance of consultation, because if the patient has been seen in the beginning by other doctors; if they all agree; if they are sure what was the matter with him, and that what ought to have been done for him, was done, this would have great weight, and will possibly prevent a suit from being brought. That I have known to be so in some cases. But you cannot allow these suits to go without attending to them, because if a person yields to blackmail, all the hungry throng of sharks will mark him out as a prey, and he will have suit after suit brought against him, and soon will be entirely ruined. Moreover, on account of his self-respect he must stand for his rights. I do not think that any suit for malpractice will hurt a young doctor: in fact, sometimes it is an advertisement. It is not altogether an unmitigated hardship.[8]

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[7] "The law requires of a man who offers his services in any profession three things: that reasonable degree of learning, skill and experience ordinarily possessed by others of his profession; reasonable and ordinary care in the treatment of the case committed to him; and the exercise of his best judgment in cases of doubt. . . . A physician does not engage to warrant and effect a perfect cure. . . . When the jury are satisfied of reasonable skill and care, that is sufficient."—*Sargent, J., Verdict for Defendant, McClelland*, pp 32, 33.

"What constitutes *ordinary* or *reasonable* care or skill, and what is the proof of it? It is not easy to say. As much cannot be expected of physicians in remote localities, where they are cut off from opportunities of improvement, as from a physician living in a community where opportunity is afforded of seeing disease and accidents under more varied forms; nor from this class as from physicians connected with hospitals, or who reside in large cities. If it were otherwise we should find but few physicians except in populous communities. The very favorable rule has been laid down in the law, that the least amount of skill, therefore, with which a fair proportion of the practitioners of a given locality are endowed, is taken as the criterion by which to judge the physician's ability or skill."—*Bouviers Inst.*, § § 1004, 1005, *McClelland*, pp. 18, 10.

"He is not responsible in damages for want of success, unless it is shown to result from a want of ordinary skill and learning and such as is ordinarily possessed by others of his profession, or from want of ordinary care or attention. He is not presumed to engage for extraordinary skill nor for extraordinary care; nor can he be made responsible in damages for errors in judgment, or mere mistakes in matters of doubt or uncertainty."—*See J. Foster*, 460 (28 Maine, 97; 39 Maine, 155); *McClelland*, p. 43.

"The defendant is not liable for want of the highest degree of skill, but for ordinary skill." —*Seare v. Prentice*, 8 East, 348: *Chitty on Contracts*, 165

"And, of course, only for the want of ordinary care and ordinary judgment. The practice of surgery is indispensable to the community; and while damages should be paid for negligence and carelessness, surgeons should not be deterred from the pursuit of their profession by intemperate and extravagant verdicts. The compensation to surgeons in the country is small, in comparison with what is paid in cities for similar services; and an error of judgment is visited with a severe penalty, which takes from one a larger share of the surplus earnings of life."—*Wells, J., McClelland*, p. 260.

We now have been over the principal points; first, with regard to medicine as a trade; second, with regard to the conduct of life in medicine as a profession. If the first lecture showed it to be pretty hard, still I trust that this lecture will show a better side. I have merely told you thus far how close you are to your patient, how near you are to the sympathies and the confidence of the community; but I have not pointed out to you that you will soon realize in practice how acute the pleasure is to you if you succeed in alleviating suffering, prolonging the patient's life, and, above all, seeing the scale turn and mount upward in a case of acute disease. To see that is most intense pleasure; and a severe case of this kind, although you may not have a great many of them at once, will give you all you want to think of until the scale is turned. You will also find among human beings with regard to the doctor, not only some ingratitude, but gratitude and friendship which will come back to you year after year, which will do you good in ways which you have not thought of, which will make you feel happy, and make you think that something you have done is worth living for. Ours is the noblest profession that exists; it is, above all, the most humane; it cannot be otherwise; we seek daily, and give our lives, to make people happier, to make them better, to alleviate their sufferings in every possible way. This reacts upon us; we share their joy; and we frequently also get more credit than we deserve for unexpected results; and although sometimes we have a great many anxieties in the event of unexpected deaths, yet these are evenly balanced by seeing others convalesce.

If any one goes into the study of medicine as a business without a positive love for it, he will be greatly disappointed; and no matter how far he succeeds in later years, yet it will all be perfunctory and mechanical. Unless one has from beginning to end a love for the profession, and a love of science, that is the only thing that will sustain one in these anxieties and disappointments; and that is the only thing that will warm one and encourage one in lawsuits under unjust accusations. After having studied medicine a little while, we find out that either we love it, or we do not love it; yet the love of science for its own sake is so entrancing and so charming, that it may beguile us on to where we shall afterwards love medicine.

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[8] In England a Medical Defence Union is in vigorous existence. It assumes the risks of suits for malpractice and defends them. It is a mutual insurance company of medical men. Here, an attempt was made in 1895 to secure the assent of the Massachusetts Legislature to the establishment of a Medical and Surgical Branch of the Employers' Liability Insurance Company. It failed, however. It is the opinion of some lawyers that the existence of such an association to defend the doctor, would turn the jury against him.

There is nothing more delightful than the study of medicine; it is very different from the practice of medicine. The study of medicine is one delight from beginning to end. You begin with anatomy, and what is more delightful than the study of that subject for a year or two, in which you can see almost every mechanical law and mechanical device demonstrated; then you pass on from that to physiology, and see all the most delicate functions; and when you go on still further and notice them in sickness, and can reason on the causes and conditions of disease far better than you could before, and then apply remedies that will regulate and give relief, what study is more delightful? The horizon of knowledge ever recedes. You are just as much a student when you are eighty years old as when you are twenty. Surgery lately has advanced so far and so fast that it has distanced medicine, and is now, since the application of antiseptics, the most attractive thing; and this, of course, is limitless; but meanwhile, even now at this moment and henceforward, I am sure that medicine is going to resume its own place, and by and by maintain its supremacy, which it always should have over surgery. I have always said that the physician is superior to the surgeon; that he has a broader field to look over; that his knowledge must be more extensive to make him a good physician than a good surgeon. Now the physician is coming back; and he is coming back through the great advances being made so rapidly through the discovery of the antitoxins and of bacteriology. In ten years, I will venture to say, that medicine will be on as high a plane as it ever was. The resources of our profession are endless in delight; and if you find in the beginning that you love it, you will never cease to be happy in its pursuit.—Boston Medical and Surgical Journal.

THE STUDY OF HYGIENE; ITS SIGNIFICANCE FOR THE PHYSICIAN, THE STUDENT AND THE LAYMAN.*

BY A. JACOBI M.D.

Hygiene is that part of medical science and art which treats of the causes and prevention of disease. We can not conceive of a subject of vaster importance and more incompatible with ignorance. It ought to be understood by every man to whom is entrusted the care of the individual or of the community, for it comprehends, as you will understand if you will but run your eyes over the index of any text-book on that subject, a great many vital topics. To it belongs the normal and abnormal conditions of air, water, foods and soil; the removal of sewage, the construction and sanitation of dwellings, hospitals, schools, manufactories, camps, arsenals and prisons; and the subject of baths and bathing, of clothing and exercise. The theory of disease germs and of contagion and infection, with the history of epidemic diseases and the disposal of the dead, prevention and treatment by antiseptics, disinfectants and deodorizers, and protection by quarantine, and finally, vital statistics form intrinsic constituents of hygiene.

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* From an address delivered at the dedication of the Bender Hygienic Laboratory, Albany October 27, 1895.

Fifty years ago when I was considerably younger than to-day, and began the study of medicine on which I continue to be engaged at the present time, Virchow had just commenced his war against ontology, and laid the foundation of cellular pathology. Still, the differential diagnosis of what has been called Bright's disease had begun to be elaborated but twenty years previously. The stethoscope was not twenty years old, and my professor at Bonn, Friederich Nasse, then three score and ten years old, was one of the few who appreciated and enthusiastically taught on German soil, this French innovation. In Goettingen I had an illustrious teacher who was at the same time professor of anatomy and dissected with us, and of operative surgery and director of the surgical clinic. In all Germany outside of Vienna, which at that time was part of Germany, there were but two places where pathological anatomy and histology were taught, and but two men who gave such instruction, namely, Virchow in Würzburg, and Frerichs in Goettingen. That is why I traveled four hundred miles to the latter place. In a New York institution I was, some thirty or thirty-five years ago, connected with a surgical gentleman of knowledge, enterprise, skill and fame, whose proudest possession was a large, splendid, gorgeous velvet gown, which covered him all over to his feet, and made a tremendous impression on the spectators while he was operating on the dead or the living; he attended to both, sometimes in the same hour and on the same table in the amphitheater. Now and then he would perform the same operation, first on the cadaver and immediately afterward on a patient. It was not very hygienic, but at that time it was strictly legitimate. A hospital and a dissecting-room were under the same roof. That was but little more than thirty years ago, and few of us knew or taught better. A short twenty-five years ago we had in connection with wounds no higher ambition than to see pus. If there was none we might have, and too often had, erysipelas and gangrene. Pus was always looked for and welcomed if it was but "*bonum et laudabile.*" When I was a student there was no laboratory anywhere but the chemical. It was at that time that Pettenkofer began in private his influential studies on topics connected with hygiene, but it took decades before he succeeded, in 1878, in obtaining an official laboratory. Still, from him and his time date the appreciation of the theoretical and practical importance of hygiene as a science. I may state the gradual effects of his personal and local efforts right here. It was from that period that the sanitation of Munich became possible. It resulted in the extermination of typhoid fever which had decimated both the home population and the strangers. It spread health and vigor over old and young, while embarrassing the mourning goods stores and the undertakers.

THE LESSONS OF WAR.

In recalling these historical facts and personal reminiscences, let me not forget, however, that within that time great achievements were accomplished even without the modern methods of work and study. Although there were no laboratories, and no high-power microscopes, no Abbe condensers and no oil immersions, there was what no great metropolitan teacher nor obscure country practitioner should be without—educated brains, powers of observation and application, and common sense. Within that very time, a century ago, Jenner had introduced vaccination, and one-third of a century ago the United States improved hospital arrangements. It is our pride as a nation, and always should be, to remember that America has been foremost in accomplishing the greatest practical progress almost without any schooling. I can do no better for you than to copy, in order to make clear what I mean to say, literally from our great pathfinder and master, Rudolf Virchow. In his "Progress of Military Medicine," an oration delivered on August 2, 1874, he says: "The French army lost in the Crimean war 33% of its men, namely 95,615. Of this number 10,240 were killed on the battlefields, and about as many died of their wounds in the hospitals. More than 75,000 men died of infectious diseases. In the American civil war 97,000 died of their wounds and 194,000 perished of infectious and other diseases. What a vast amount of pain and misery! What an ocean of blood and tears, and besides what a number of errors, mistakes and prejudices! It is not necessary to now enumerate the long list of blunders and sins. They are so well known as to serve in the future as warning examples. Let me say here that it was not misfortune alone that showed where the cause of evil was and then provided aid. If the French learned little or nothing in the Crimea, and the Americans so much in their civil war as to create a new era in military medicine, the explanation is not to be sought for in the immensity of misfortune and misery undergone by the Americans, for they did not suffer any more than the French did in the Crimea. The explanation is in the critical and thoroughly scientific spirit, the clear perception, the sound and practical common sense which penetrated gradually every part of the American military administration, and which with the astounding coöperation of the entire nation accomplished more humane results than any great war ever produced before. Whoever studies the copious publications of the medical staff of the American army must again and again be astonished at the vast experience collected in them. Absolute accuracy of details, the most painstaking statistics, acquaintance with all branches of medical learning, and a comprehensive style are united in them for the purpose of collecting and preserving in the interests of the present and future generations the new knowledge so dearly bought."

The practical results of the civil war were the pavilion system, increase of air space, the isolation of contagious diseases, the improvement of heating and ventilation, and thereby a vast saving of lives. The lessons were not lost; were learned and adopted all over the globe, and made the American name honored and blessed all over the world. Understand, young men, for the present and the future, that this is the only practical and useful sort of interference with European nations which is worthy of the American people and consistent with the principles of our government.

PRIMITIVE IDEAS OF DISEASE.

While almost instinctively we succeeded in finding some way of escape from deadly diseases, the Europeans advanced slowly and surely to the evolvement of what first appeared theoretical science only, but proved of vast practical import. These two, as a rule, go hand in hand. It is true science that has its own reward in satisfying the mind, but I know of no instance in the history of mental development but demonstrates the fact that the most theoretical, the most abstruse intellectual result will always turn out to be serviceable to mankind. Of this there is no more beneficent instance than the methods by which the sources of disease have been fathomed. By the study and knowledge of their causes the greatest improvements have been made in the prevention and treatment of epidemic diseases. There was a time when they were unexplained in their suddenness and considered unavoidable. Niobe and her nine children slain by Appollo and Diana are always sad realities. There have been millions like them, though they alone live forever in marble and prove that the highest degree of æsthetic and artistic achievement may well accompany dense ignorance. That may occur in nations as it does in individuals. In heathen antiquity and Christian mediævalism disease and death are deemed punishments. There is no way of combating them;

mute submission only is demanded. Nothing is indicated but prayers, offerings of beast or man, and pilgrimages. There was another time when nothing bad could happen except through some evil influence. Somebody was at fault when a cow or a child died. There was witchcraft or the evil eye. When there was a general dying somebody must be killed for it—dissenting Protestants by the hundreds, Jews by the thousands.

There was a later period when it was neither gods who sent their swift arrows nor Jews who poisoned the wells; when it was well known that epidemics would follow in the path of wars and thus depopulate the large cities; that they would spread from man to man and thus decimate the people. The latter belief was even exaggerated when it was found advisable to class syphilis also among those diseases which could be transmitted without physical contact, for nothing is more pleasing to human self-love than to escape, not so much guilt as the appearance of guilt. Thus it was felt that in the dissemination of contagious diseases there must be something the nature of which was unknown and unknowable. The "genius epidemicus," the epidemic tendency, was the pass-word which allowed etiology to go unchallenged. By others, though they admitted not to know the nature of the contagion, the latter was believed to originate in the body, at least in some instances, spontaneously. Typhoid fever, tuberculosis, dysentery were thus explained. By many, spontaneous generation of the cause of the disease was assumed for the generality of cases; by others, for the first instances, of the epidemic only, but no matter whether the real cause was hidden there were the facts of contagion and of awful mortality; and the clear head of the eighteenth century—Sydenham in England, Boerhaave in Holland, Van Swieten in Holland and Austria, Peter Frank in Italy and Austria—knew better than to cross their arms in such nihilistic contemplation, as reigned supreme through a protracted period of this very century of science and humanitarianism. Many of the the measures established at present on solid knowledge and improved by experimental research were inaugurated by them. Their reasoning was as follows: Man is the carrier of contagion; therefore, when falling sick with a contagious disease he must be isolated. A contagium will not cling to everybody indiscriminately. Thus the amount of individual vulnerability appears to be larger or smaller, but no matter how various the degrees, there is some vulnerability probably in everybody. Therefore, try to diminish it by saturating the system with the poison when the epidemic is mildest. Thus they inoculated variola and risked a slight danger rather than allow men to die of a greater. And thirdly, no matter whether the nature of the contagion is clearly known, it is advisable to destroy or diminish its poison. As early as the period of Black Death in the fourteenth century people would keep wood and tar fires burning in the streets. Was it the instinctive belief in the disinfectant action of heat, of tar, and of creosote?

Afterwards fumigations with vinegar, with chlorine, nitrous acid, were resorted to. Patients and utensils had to submit to disinfection as best they could, and goods or tools were either burned or temporarily buried.

THE ORGANIC ORIGIN OF DISEASE,

which was to be proven after manifold labors, was first suspected in antiquity when imagination gathered all the visible and alleged invisible insects under the sinister command of the supernatural patron, the devil. Amongst the Romans, Varro suggested the presence, like that of big insects, of invisible minute beings hovering over swamps which he thought might cause fevers. Paracelsus when speaking of the seed of diseases may have believed in similar hypothesis. A clergyman, the Jesuit Anastasius Kircher, 1671, was the first, however, to claim contagion to be the result of organic influences, and Leuwenhoek being in possession of a better instrument made by himself, was the first to discover real bacteria.*

Progress, however, was slow. It took a century until O. F. Müller, 1786, was able to describe and draw some of them. The names given by him are still in use. Bory de St. Vincent in 1824, Ehrenberg in 1838, Dujardin in 1841, added a great deal to our knowledge. In my own time Perty, in 1852, discovered the changes which take place in bacteria when developed under varying circumstances, and in 1853 Ferdinand Cohn classified, rather pedantically, it is true, the whole number of microbes then known.

Kircher's theory of the organic origin of disease was adopted by Lancisi, Reaumur and Linnè. Its firmest believers were Marcus Antonius Plenciz whose book of 1762, and Pringle whose researches on infectious diseases and disinfectants furnish the best presentation of the subject in the last century. Their views, as Ferdinand Hueppe aptly remarks, were, however, not generally held because it was not so much the cause of the disease as the disease itself that was considered to be a living parasite; for it is from that time that such names date as lupus and cancer, which have outlived their justifiable term of existence. Besides, positive knowledge based on descriptive observation was defective to such an extent that even Malpighi, Morgagni, Bichat, and up to 1874 Virchow, gave their attention exclusively to the pathological anatomy of organs and tissues.

At that time, however, the question of the nature of putrefaction engaged the attention of many minds. Decomposition was proved to be organic in yeast by Astier in 1813; the so-called "bleeding hosts" were demonstrated by Sette in 1816 to owe their discoloration to bacteria. In 1837 Cagniard Latour studied the yeast deposits which he found during alcoholic fermentation. He proved them to be organic, and presumed they were the cause of fermentation. In the same year Schwann became more positive of this, and moreover found that extract of nux vomica did not stop fermentation, but that arsenic did. This is why he took it to be of vegetable nature. The same results were obtained by Turpin in 1838. Fuchs in 1841, Remak in the same year, and 1843 Mitscherlich referred all fermentation to living microbes. The last two named authors were the first to find two different microbes in two different forms of fermentation. Helmholtz also participated in these researches without, however, excelling here as he did in almost everything else he undertook. It was only in 1857, however, that Pasteur could prove undeniably, in spite of Hiller's and others' remonstrations, that all putrefaction and fermentation were attributable to microbes, and that each different fermentation had its own specific organic cause.

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*J. Ferdinand Hueppe, Naturwissenschaftliche Einführung in die Bakteriologie, 1896. Friedrich Lœffler über die Fortschritte in der Bekämpfung der Infektions Krankheiten in den letzten 25 Jahren, 1896. Max Gruber, Pasteur's Lebenswerk, etc., 1896 Charcot, Bouchard, Brissaud Traité de Médecine, Vol. 1, 1891. G. M. Sternberg, A Manual of Bacteriology, 1892. Victor C. Vaughan and Frederick G. Novy, Ptomains, Leucomains, Toxins and Antitoxins, 3d Ed., 1896.

Long before researches on the origin of the diseases of animals, Provost found in 1807, and deBary after him, the causes of diseases of plants to be microbic. Bassi and Balsamo discovered in 1835 the microbe of the "muscadine" disease of the silk-worm, and Audouin the spores which enable the parasite to live through the winter. Schoenlein found the achorion to be the cause of tinea, Simon the acarus of the itch. It was shortly before this time that Henle, in 1840, formulated his conviction that infectious and contagious diseases were the result of living causes. His main reasons were these, that in such cases the morbid matter was evidently increasing from the moment of invasion. Organisms only have that faculty. Another reason was this, that the quantity of the invading material was out of proportion to the effects gradually obtained by it. Further, that there was a stage of incubation which was not required in poisoning by a definite dose of a simple chemical substance, and lastly, that the poison which is introduced in a very small quantity must increase before the disease can be expected to make its appearance.

Henle's theory suggested by the facts hitherto known, and supported by the published reports of Franz Schulze in 1836, and Schwann in 1837, was confirmed by Schroeder and von Dusch, 1854 and 1861, by von der Broek, 1857, and by Pasteur. Pasteur having established as a sound fact the principle, *omne vivum ex vivo*—that is, nothing that is alive can possibly have come from what is dead—and thereby annihilated the doctrine of spontaneous generation, and having further found that fermentation and putrefaction took place through external microbic influences only, Joseph Lister concluded that if wounds became infected through such living external influences only the latter should be kept away from them before they could reach them or be destroyed. That was in 1867.

ACTIVITY IN THE STUDY OF BACTERIA.

There was great activity in the study of bacteria from 1866 to 1876. Klebs found microbes in pyonephritis in 1866, Rindfleisch in pyæmia in 1856; soon after Recklinghausen and Waldeyer in metastatic abscesses, Klebs in gunshot wounds in 1872, others found them in septicæmia, erysipelas, hospital gangrene, puerperal fever, diphtheria, and finally, 1873, Obermeier in relapsing fever. In this way much valuable material was collected, but unfortunately but little sifted. It so happened that in similar conditions many varieties were found to occur simultaneously, by several in abscesses, also in diphtheria, in cholera, in variola. Pasteur found a number of bacteria in putrid material, but in most cases no positive decision was at that time arrived at as to the relationship of specific microbes or poisons to specific diseases. Besides, there was never such a scramble for notoriety as in those times. Dilettanti in journalism were as numerous as pickpockets at a fair or tramps after a battle. The medical weeklies and monthlies of that time teemed with new discoveries and new bacteria. Bacteria found in the dead body were proclaimed to be the cause of death, accidental finds the origin of a special disease; and hundreds of essays appeared at that time which proved nothing but carelessness in experiment or observation. Thus it could happen that even all the results obtained by one of the most active and ingenious experimenters, Hallier, could be proven by de Bary to be worthless, because no protection had been procured against the accidental admixture of miscellaneous bacteria. At that time it took a great deal of mental equilibrium not to be swamped by the flood of new microbes. Indeed, in spite of the results already attained and appreciated, the profusion of immature publications was enough to create doubts and prejudices in the minds of many clinicians. It was at that time, in 1880, when finishing my treatise on diphtheria that I availed myself of that occasion to express my own feelings about the hasty and immature conclusions arrived at by the crowd of microscope fiends and leaned in the explanation of the diphtheritic process rather to a chemical poison than to the influence of a parasite. Riper experience, all built up on laboratory work, has since taught us that in diphtheria as well as, with but few exceptions, in every infectious disease in whose development microbes are known to be concerned, it is a chemical product of the microbe only which penetrates into the circulation and destroys life. It is fortunate indeed that it should be so, for chemical poisons are more easily counteracted than living microbes circulating in the blood. The latter condition is present in anthrax. That is why Pasteur's inoculation of thousands of flocks of sheep with less virulent anthrax has not proved an unmitigated success. Deaths have taken place after such inoculations as they did when in the last century smallpox was inoculated to protect against smallpox. In southern Russia in such attempts at conveying protection thousands of sheep were destroyed. When speaking of this accident it was Haffkin, of whom I shall have more to say, who, shuddering, excleaimed "If that happened to man!"

After many mistakes, amongst which one of the most prominent is the alleged isolation of Bergmann's "sepsin," Panum proved that septic poisons could be derived from microbes, after Coze and Feltz in 1866, and Burdon Sanderson, and in 1872 Davaine had succeeded in perpetuating infectious processes by persistent inoculations of the same microbes from animal to animal. As early as 1868 glanders had been produced by vaccinating its pus, and it was proved by the effects of immense dilution that the infecting material must necessarily be of organic nature. Still, the nature and manner of propagation of the microbe was not recognized. Finally in 1876 Koch discovered that the great vitality of the bacilli of anthrax, themselves very perishable, owed their vitality to the formation of spores.

Thus far the parasitic theory appeared to be rather firmly established, but still, as many believed in the excessive variability of bacteria, and attributed their many different forms to transmutation of a single original form, they were often believed to be not the cause, but the accompaniment of the infectious process. All this happened in spite of the fact that Pasteur had differentiated the bacteria of wine and of beer fermentation, of anthrax, of sarcina, of amylobacter, and Obermeyer's specific spirilum of relapsing fever was known since he paid his self-sacrificing zeal with his own life. Another proof of the essential differences of bacteria was accidentally found by Schroeder, who noticed differences in the color of his cultures, but a scientific differentiation became possible only when C. Weigert and later on Ehrich taught the systematic staining of bacteria by means of basic aniline dyes. With this method and better microscopes, and immersion, and the use of Abbe's condenser, Koch made rapid strides. In 1877 he published his treatise on the methods of research, and in 1878 the etiology of the infection of wounds. About the same time Pasteur, 1877, after having proved in 1863 that normal blood was free of germs, demonstrated that the bacillus of anthrax would grow not only in the circulating blood, but also in blood outside the body and on other nutrients. Thus prepared, he and his pupils discovered a number of characteristic microbes: that of malignant œdema in 1877, the staphylococcus pyogenes in '78, and that of sputum-septicæmia, identical with the pneumococcus of Fraenkel in '80, of chicken cholera in '89, of hog cholera in '82. Lister, Buchner and Koch further improved the methods of examination until in 1882 the latter succeeded in demonstrating his bacillus of tuberculosis before the physiological society of Berlin. At this time it was conclusively proven that the microbes had each their peculiar etiological significance, that in fact it was a single specific microbe which caused a single specific infection or contagious disease.

THE DISTINCTION BETWEEN CONTAGION AND INFECTION.

Let me here try to correct what I believe to be a mistake and to give rise to confusion. The terms contagion and infection are too often used promiscuously, though they are by no means synonymous. The dissemination of mycotic diseases takes place in different ways. There are those which cannot be communicated from person to person, but spread only by the microbic cause invading the individual. To this class belong malarial fevers produced by plasmodia. Malaria is not contagious; just so, in all probability, cerebro-spinal meningitis. There are, secondly, those which are not communicable from person to person, but through external carriers only, such as soil, water, food, air, clothing and utensils. To that class belong yellow fever and Asiatic cholera. They are infectious. There are, finally, those which may be transmitted directly from a person or indirectly through carriers. To this class belong scarlet fever, measles, diphtheria, variola, influenza, erysipelas and varicella, perhaps also whooping cough. They are contagious and infectious. As its cause has not been sufficiently proven to be microbic, I do not add syphilis. On no account, therefore, ought the terms contagion and infection be taken as synonymous.

PASTEUR'S GREAT WORK.

The belief of Naegeli that the multiplicity of bacteria originated in a single or a few changeable varieties was proven to be erroneous partly by direct observation and partly by the results of cultures undertaken under varying circumstances. It can be demonstrated that the infectiousness of bacteria may be increased or lowered. That is what Buchner found in 1880 in regard to the bacillus of anthrax. The main progress in the following years was made by Pasteur, either single-handed or in coöperation with his pupils. In 1880 he found accidentally that the bacillus of chicken cholera was less virulent when the cultures were exposed to air and got cool. Still, the hens infected with them became ill. After they recovered he infected them with thoroughly virulent germs, and, lo and behold! they were immune. From that time dates all our modern protective and curative therapeutics of infectious diseases. Pasteur then experimented with anthrax. A few of his results can be stated in the following brief sentences. The bacillus of anthrax did not develop in the blood of the living chicken. That is, the latter were immune against anthrax. But the bacilli did develop in the blood of chickens outside their bodies. The conclusion was that the presence of bacilli in the animal body was active or inactive, not through its own properties, but only so far as the animal tissues permit of its influence. The disease, therefore, is not established by the presence of the bacillus, but is the composite of the symptoms caused by the battle of the invader with the living cell.

Pasteur also found that the infected chick remained immune when retaining its own high temperature. When it was cooled by being immersed in water it fell sick, but recovered when again warmed. The conclusion is that the development and growth and injuriousness of bacilli, or some of them, depend on the temperature of the medium in which they are. He further found that the bacillus of anthrax grew in sterile urine when being alone in the fluid, but not when other bacilli were exposed together with them. This observation was confirmed by further experiments. When these bacilli were injected with other bacilli into an animal, the bacilli of anthrax did not thrive, and the animal remained alive and well. These discoveries followed one another in quick succession. Septic phlegmonous suppurative processes of all kinds were found to be caused by different bacilli. On the other hand, the same microbe would cause different affections in different animals. The bacillus of chicken cholera, for instance, produced general sepsis in the chicken, but a local abscess only in the guinea pig.

The virulence of the bacillus or its poison can be lowered in many ways—by high temperature, partial desiccation, dilution by chemicals, or by culture of the original in different animals. Pasteur's experiments with the bacillus of anthrax were very numerous and efficacious. He produced cultures of lowered infectiousness and vaccinated sheep. It is true many died of the infection, but a great many were rendered immune, and the mortality from anthrax was very much reduced. Experiments made with the view of lowering the virulence of bacilli in other animals were also successful. That of hog erysipelas was thus changed when passing through the rabbit, and the poison of rabies, believed by Pasteur—though even he could not isolate it—to be the product of an organism, after unsatisfactory experiments with monkeys, was weakened by its passage through the same animal, viz., the rabbit. The indomitable man was successively engaged in finding new hypotheses based on well-known facts, and in experiments which were to prove or disprove the value of his hypotheses. In regard to rabies his reasoning was as follows: The long duration of its incubation proves the slow progress of the poison in the direction of the nerve centers, for it is of symptoms of the nerve centers that the patient infected by a mad dog finally dies. Thus the time between the infection and the affection of the nerve centers should be utilized by antitoxic injections. The poison must be intercepted. It is well known that the spinal cord of the rabbit is gradually and in proportion to the duration of its presence in the organ changed by injection with fully virulent rabic poison, that the different degrees of morbid changes produced in its tissue yield a material which is employed for injection into the patient who has been or is believed to have been infected. The attacks on this method, or rather its effects, are many, but it appears that the good done by it far outweighs its dangers or ill results. And one fact must never be forgotten—it is but little more than a decade since the method had first been employed. It has been the forerunner of other discoveries of similar import, and of more and extensive utility. Thus it appears that the future therapy of infectious and contagious diseases will be built upon the foundation laid by the work performed in Pasteur's and other laboratories.

As far as Asiatic cholera is concerned, the discovery of the comma bacillus appears to have finally settled the question of its cause. The bacillus, however, does not enter the circulation, but destroys life by secreting a deadly poison. So does that of yellow fever; so does rabies; each with its own poison bred by its own specific bacillus. In these diseases it is not the weakened bacillus that can be expected to act as an antidote, but the lowered strength of the chemical poison. Thus it was that Ferran's therapeutical experiments with living cultures in 1885 were of doubtful importance. When cholera again appeared, in 1890, Haffkin of Russia, working in Pasteur's laboratory, found that cholera bacilli, when cultured, lose part of their activity. Imbued as he was with Pasteur's principles the light flashed upon him. The lowered activity of the cultured baccilli could be expected to be utilized as the healing agent after having been tried on somebody. But animals are immune, so he made himself the object of experiment, and he did not die. Within two years afterwards in the East Indies he made 70,000 injections on 42,179 human beings in 98 different localities. No accident marred his operations, though pain followed each of them for four or five days. In order to control his experiments he always vaccinated a part of the population only. What were the results of his laborious and self-sacrificing labors? In Calcutta the morbidity of those vaccinated fell to 1-19 or 1-24 compared with former experience; the mortality fell to 1-17 or 1-24. If in the near future—in Berlin they are experimenting at this moment on the action of dead cultures which appear to immunize—the methods will be improved, and the East Indian and Abrabian pilgrims be vaccinated before they start for Mecca, "Death, where is thy sting! Grave, where is thy victory!"

THE SPECIFIC INJECTION OF DIPHTHERIA.

As so much has been written on the subject of diphtheria, a few words may suffice on that form of this contagious and infectious disease, which is caused by or, as some still say, accompanied with the Klebs-Loeffler bacillus. The bacillus locates on the mucous membrane of the naso-pharynx, sometimes on wounds. On the mucous membrane it produces a disease only when the surface is sore, and thereby amenable to invasion. It does not enter the circulation, but produces a poison which is rapidly absorbed. When this chemical poison is injected into an animal in slowly-increasing doses, which should be large enough to make sick, but not to destroy, an antidote to the original disease is produced in the serum of that animal. This is the healing serum of Behring, which has succeeded in greatly reducing the mortality of diphtheria, and may be further proved to cause temporary immunity when injected into those who are well, but constantly exposed. This almost fabulous result is the result exclusively of laboratory work. The success accomplished thus far speaks well for the future. It does not require an unbounded enthusiasm to believe that the results will be still more favorable in this dread disease, which destroys the infants and children in every country by the ten thousands, and to trust that further study and experimental labors may find similar methods for the subjugation of other contagious and infectious diseases.—The Dietetic and Hygienic Gazette.

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PROCEEDINGS OF SOCIETIES.

RICHMOND ACADEMY OF MEDICINE AND SURGERY.

Regular Meeting Held March 9, 1897.

Dr. J. N. Upshur, President, in the chair; Dr. Mark W. Peyser, Secretary and Reporter.

The President read a paper on the "Therapeutic Application of Chloroform in Labor." [See Page 273.]

DISCUSSION.

Dr. J. S. Wellford did not agree with the speaker. If women of the present day were in a condition as perfect as those of the primeval state, there would be no place for chloroform; but, as she now is, it is indicated. He never gave it till labor was far advanced in the second stage, and he got the woman thoroughly under the influence, withdrawing it until pains occur again. Ergot was administered twenty-five or thirty minutes before the time of delivery, and when the effects of chloroform were passing off Thre was a greater tendency to hæmorrhage and slower labor when the anæsthetic was given. He, therefore, exhibited ergot in time to exert its physiological effects. Half of the mental depression was removed by chloroform, and that in itself was valuable. Take a delicate woman and subject her to the same amount of pain from other causes, and the idea is awful. There was much less difficulty in convalescence with, than without chloroform; there was not half the danger of child-bed fever, and shock was not as great. It was the exception for him not to give chloroform, unless the case was a short one.

Dr. John F. Winn thought a great deal depended on whether the chloroform was pushed to the obstetrical or the surgical degree. If the former, and given just before the pains in the second stage, he could not see how any bad results could occur.

Dr. Wm. S. Gordon remarked that as he understood it, the question was whether chloroform should be given in normal labor. In his experience, a large number of young women had prolonged, nagging pains because they had listened to the tales of friends, or had had access to medical works. In many cases, he never gave a drop of the anæsthetic, but wherever there was a truly pathological character of pain, as prolonged nagging, etc., he did administer it. When used, it was liable to cause uterine inertia and hæmorrhage, and indications for forceps were often. It was a moral question: Shall we annul the pain of normal labor?

Dr. Landon B. Edwards gathered from that portion of the paper he had arrived in time to hear that the question was whether or not chloroform should be used in natural labor. All admit its utility in instrumental deliveries and in most of the dystociæ. "Natural labor" is more or less a relative term. For instance, it may be over in three or four hours, or it may last twelve or fifteen hours. Again, it occurs in women of markedly different temperaments. We have seen labor progressing naturally—with proper dimensions as to mother's pelvis and child's head and body—in determined, resolute women, who had the courage and strength to bear their pains well and to obey the instructions of the accoucheur. Dilatation may be occurring gradually enough to permit the birth of the head and shoulders without tearing more than the fourchette or causing only an immaterial slit of the vulvar margin in its distended condition. A few such cases never ask for, nor wish, nor need chloroform. But the vast majority of women are not so resolute; many have a decidedly nervous temperament, and cannot well endure pain—they suffer inordinately as normal labor progresses. Many such persons—even during natural labor—become uncontrollable; and unless the anæsthetic is promptly and thoroughly administered, a number of dangers may threaten. So that there are cases of natural labor which almost imperatively demand an anæsthetic. Then, again, there are some patients who do not seem to act well under an anæsthetic—it makes them sick, or it relaxes too much uterine contractions, or otherwise too long retards labor. But such cases in his experience are also comparatively rare. So that, the question as to whether or not chloroform should be used is not to be decided by a statement that labor is progressing normally, but by a recognition of the traits or temperament of the patient in hand. It is principally a personal equation—it should not be decided until we come to the case. If the suffering is intense, and if the patient bears the chloroform well, it borders on cruelty not to let the sufferer have the benefit of anæsthesia in labor. There should never be occasion for the text at the funeral of the true physician: "I was in distress and ye comforted me not."

A great deal is said as to the degree of anæsthesia in natural labor. In his earlier experience, he was often frightened off by the dictum: "Just enough to take the sharp edge of pain away." Now, just this much may or may not be enough to make the sympathetic physician feel that he has done his duty. He has often seen "just so much" leave the reflexes in a state to excite contraction of the sphincters of the vagina, and thus resist dilatation, which was overcome by profound or surgical anæsthesia. On the other hand, he has several times seen a condition of temporary inertia of uterine action induced by such degree of anæsthesia which retarded the progress of normal labor. He knows of no way to decide beforehand which of these two extremes of action—if either—chloroform may excite. But his experience has long since taught him that it was safe to test the matter by experiment, for in by far the majority of instances in judicious hands, chloroform will act just right in labor. In a few minutes, however, the lesson will sometimes be impressed that neither degree of anæsthesia is adapted to the case in hand, and we are thus forced to give up the further use of chloroform in these rare instances.

When chloroform acts well in normal labor, as it very generally does, he regards it as a matter of importance to maintain anæsthesia until the moment of emergence of one of the shoulders; for by common agreement it is then that perineal rents are most frequent and severest. If chloroform relaxes sphincter contraction, and thus permits of greatest dilatation, it is just at this moment that anæsthesia should be most profound. As soon as the head is born, the cord slipped from around the neck, etc., if apprehension exist that the shoulders will materially tear the perineum, promptly try to deliver that one which seems easiest, while the head and then the body are brought well up on the abdomen—thus prolonging the axis of the curve of Carus. Passing two or three fingers in the vagina down to the posterior shoulder with their palmar surface pressing upon the posterior vaginal wall, promoting distension, and, at the same time, aiding extension of the perineum as the pain comes on, very decidedly prevents the dip of that shoulder behind or under the ring of muscles which appear as a cord covered by mucous membrane at the ostium vaginæ just after the head is born; and thus enables the shoulder to escape, in case of profound relaxation by chloroform, without any material rent. In fact, he does not recall when, in such cases of normal labor under chloroform, he has had a tear of the perineum of sufficient extent to need a stitch; although he has often to sew up split perinei after some dystociæ and nearly all instrumental labors.

In speaking of profound anæsthesia, it is scarcely necessary to say that it need not at any time be prolonged beyond a few minutes at a time. In other words, suit the amount to the condition to be met. Off and on, he has often kept patients in natural labor, under the anæsthetic five or ten minutes at a time. Sometimes he has to sprinkle cool water over the surface of the child as it lay between the mother's thighs, to induce it to gasp, but he recalls no instance where the chloroform narcosis of the child was marked enough to prevent its living after its first breath.

As to the after-effects of chloroform upon the mother, he is satisfied that they are very much overrated. The worst post-partum hæmorrhages he ever had, occurred in two ladies neither of whom had taken a whiff of chloroform. In both of these cases, the first indication of such hæmorrhage occurred fully an hour or more after delivery of the placentæ. The practitioner should not assume the duties of obstetrician unless he has a patient disposition and is willing to take the time to attend to every detail of a labor case, and be within speedy call of his patient for full an hour and a half or two hours after her delivery. Proper attention to the condition of things during and after labor under chloroform had saved him thus far from post-partum hæmorrhages, subinvolutions and other accidents. He is confident that many times convulsions have been prevented by chloroform. So that an answer to the question of using an anæsthetic during normal labor resolves itself into determining whether or not the patient in hand can get on best with or without it—the great majority of patients, according to the experience of the speaker, being benefited rather than injured by the circumspect use of chloroform.

Dr. Virginius W. Harrison thought that the shoulders did more injury to the perineum than the head, and he continued the use of chloroform till the shoulders were born. He gave quinine ten minutes before beginning the anæsthetic. Unless pains were severe, and there was no dilatation, he waited some time before using it, and then just sufficiently so that the exquisite pain on passing the os could not be felt. If pains mitigated, he withheld, renewing the administration as needed.

Dr. J. Allison Hodges said he ruptured nearly every perineum he attended. He asked Dr. Edwards to what extent he carried anæsthesia.

Dr. Edwards replied that he generally gave chloroform till the shoulders were delivered.

Dr. Hodges said he was surprised at Dr. Edwards' experience, and that he doubted that any other gentleman present had the same, and he said it with all due deference. He, himself, seldom gave chloroform at the last stage when the child had been born, for he did not believe it correct. The relaxation was such that if hæmorrhage occurred, he would blame himself. That chloroform was to be given in normal labor, depended upon what we called normal labor. In some cases, he would give it; but where the woman had the courage and the fortitude to bear the pains, he would not. Using the agent just a moment before the pain, we could mitigate its severity, and then take it away. When the woman laid for a hour in a stupor, after the child was born, he thought we overstepped the rights of the patient, and used something that did not belong to us.

Dr. Harrison said he had never seen chloroform effects last an hour.

Dr. Hodges said his point was the great relaxation and the probability of post-partum hæmorrhage, unknown when the woman was in such a condition.

Dr. Wellford thought that in a number of cases, chloroform hastened labor by diminishing the impediment in front in a greater degree than the lessening the vis a tergo.

The President, in closing the discussion, said he had expected criticism for the views expressed. The various speakers had begged the question. The point he had made was, "Is chloroform justifiable in natural labor progressing with satisfactory rapidity?" Of course, he always carried strychnia, etc.,—when there was indication for it. He had been taught to use chloroform; but long experience and constant observation of its effects in retarding labor, had induced him to call attention to its baneful effects. He had a patient whom he had delivered a great many times. Labors without chloroform were natural and rapid, but she invariably had inertia of the womb and instrumental delivery whenever he had given her chloroform. He delivered last summer a lady of her fourth child. She gave a history of instrumental delivery in the preceding labors, which he could not understand, as she was in vigorous health, with large, roomy pelvis. Enquiring of a gentleman who had twice delivered her, as to what the trouble was, the reply was, uterine inertia. She had always taken chloroform. When labor came on, he gave muriate of quinine, grs. x; strychnia, gr. 1-20; and in three hours, strong bearing down pains came on, and she was promptly delivered naturally. She had begged earnestly for chloroform, but afterwards acknowledged that she had had the best labor, and getting up sooner than ever before.

He wished to protest against the use of chloroform in every case, because it is a fashion and demanded by our patient, as being harmful, unless there was some special indication for its exhibition.

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ABSTRACTS.

SPECIALTIES AND MEDICAL ADVERTISING.

Perhaps nothing is more remarkable in civilized human nature than that peculiar form of hypocrisy which enunciates certain laws for general guidance, which the individual is at liberty to transgress, however, with entire impunity. So long as the individual proclaims that the law is an excellent one and that it should never be broken, he may break it himself with little fear of the consequences.

This inconsistency is nowhere more exemplified than in the methods of medical advertising daily in evidence among reputable practitioners in this country. As we look over the field, it would seem that every medical man was burning with a desire to advertise himself and merely lacked a safe opportunity to accomplish his object; yet not one dares to appear before any County Medical Society and advocate or attempt to justify what he himself as well as many of his fellow members do not hesitate to do whenever a pretext is furnished them.

No one appreciates more than we the splendid and generous work done in our medical societies, both general and special, by men whose object is unselfishly to add their quota to our common arcanum of knowledge; strong in their appreciation of their heritage as physicians and of their duty to their own and future times, these men make of these associations the well-springs from which we all draw sustenance and vigor. Yet, notwithstanding our appreciation for the good and honest work from which we really benefit, we can not blind ourselves to the fact that in all medical societies the most persistent exhibitors and debaters, with few exceptions, may be divided into two classes; those who have nothing new to say but who say even this for the sake of having their platitudes appear in full in the journal which reports their society's proceedings and those who pretend to have something novel to report or exhibit to the world at large and who perform this unselfish task with precisely the same object in view, namely, free advertisement in a medical journal. The apparent difference between these two classes is in reality not very great, for both appear unfailingly on every occasion which promises an audience and both discuss every subject in an authoritative way which would be ludicrous were it not continued *ad nauseam*; both classes are united, however, in the common object not of advancement of medical truth but in that solely of personal advertisement. The latter class, the members of which soon acquire prominence as being "active" in their society is by far the more dangerous; they are, as a rule, rather clever, they are nearly always hard workers (which is always an accepted point in their favor) and, having formulated to themselves a clear "plan of campaign," they are usually good and ready speakers. Thus they easily deceive a certain number of ingenuous minds who are willing to accept things for what they seem. But their "trail," by which they may always be distinguished from those reporters of cases and exhibitors of specimens whose motive is not a purely sordid one, is easily followed. It has this marked course and characteristic: a certain novel procedure or method of treatment is brought forward by such an one and earnestly advocated, with a preliminary report of a number of cases; at the next meeting the same subject is again brought to the fore with a report of more successful cases and probably a number of "cures" are awaiting a committee of inspection in an adjoining room. So far their method does not differ from that common to all medical men who desire to bring a matter of importance before the profession. But it is after this that the peculiar method which we describe becomes apparent. In the latter case, the subject in question is brought up, new cases are reported and fresh discussion is excited, meeting after meeting, until public interest begins to flag; then it is dropped by its author and never referred to again. But, in a very short time, he is again to the front with an entirely new procedure and the same course is gone over until he finds it to his advantage to drop this also and find another excuse for keeping himself for a time before the medical public. By these means of systematic advertising, he usually succeeds in making a meretricious reputation frequently of wide extent, and yet it is gained without the contribution of one original thought or the enunciation of a single fact by which humanity is benefited. We have all watched such men rise from absolute obscurity and extreme poverty to extensive reputation and a large and wealthy practice, merely by this method of self-advertisement in medical societies.

Another form of advertisement, which has already become so common as hardly to excite remark, is that of newspaper interviews. Nobody to-day imagines for a moment that these are not at least indirectly solicited and yet such a suggestion would doubtless be very indignantly repudiated by these victims (!) of journalistic snares. Nevertheless, the facility with which reporters for the daily press obtain long interviews with more or less well-known members of the profession upon every subject however remotely connected with medicine or health, with which the public mind is at the moment occupied, is remarkable. The manifold consumption "cures," cancer "cures," the various subjects of serum therapy, bicycle riding for men and women, horseback riding, the comparative advantages of different makes of bicycle saddles, appendicitis, the superior value of a specified health resort—all are placed under contribution for personal advertisement and consequent increase of their office practice by medical men. How will the excuse prevail that the public need and demand enlightenment upon these subjects? If this were so, why is the required information not obtained from the faculty, as a whole, of our leading medical colleges or from the executive committee of our various medical societies, speaking for and in the name of their associations? Surely, in a matter of great and public interest, information could be obtained in this way, would be representative, would be authoritative and far more conclusive than the offhand (!) statement of opinion by any individual physician.

We pass by without especial comment the method of advertisement by testimonials to medicated wines, cordials, proprietary medicines and surgical appliances—these all bear the indorsement and names of prominent medical men with their respective addresses. We would not be very much surprised, indeed, if some day we should see on a full-page advertisement the smiling features of some medical man bursting through a cardboard, with the cheery salutation and familiar question: "Good morning! Have you used Pear's soap?" Of course this would be accompanied by the legend: "This is what Dr. X. says" (name and address given).

All this is against not only the spirit but even the very letter of that law universally upheld among medical men as embodying the distinctive and vital difference between our noble profession and that occupation of the majority of men, which consists merely in buying and selling and is called trade. Why may this law be daily broken with impunity? Because the methods of self-advertising we have described are not labeled such; they are called by any other name and thus the medical conscience remains at peace.

We also believe fully in the justice and necessary maintenance of this law, but we believe that it is far more important to follow it in its spirit than in its letter alone. What is this law and what does it demand of us? That we should not announce our talents among the laity, however great we believe those to be nor however much we covet appreciation, either verbally or in print. In other words, we are forbidden to place our own valuation publicly upon our medical wares, as mere traders do, but are commanded to work and wait that the public "may know us by our fruits." This is the law, properly interpreted both in spirit and in letter; yet as we have seen it is constantly broken unblushingly and with impunity by many prominent medical men. But let the word "advertisement" be used in conjunction with the name and special work of a practitioner, even in a medical journal, and many would cry out that the code of medical ethics had been ruthlessly violated. Here again is a great inconsistency. It is altogether permissible that a so-called specialist should announce, before a few unbelieving members of his society, who know both him and his work, his wonderful methods and wonderful statistics for the gaping information of the many far-distant readers (who do not know him) of the medical journal which will report his words. This is allowed. Why then is it not permissible that reputable specialists should insert regularly paid advertisements, announcing that they are specialists, in the advertising columns of medical journals? We believe it should be. Not only this, but we further believe that such a course would be greatly to the benefit of all specialists, if such a custom prevailed. It would have the effect of separating the true specialists—those who really practice what they preach—from those who speak with the authoritative utterance of specialists before special societies, but whose very general practice in reality is as far removed from that of a specialist as is their previous special training. These latter are known only as specialists to the readers of special journals. Their immature opinions are given the same weight as those of the true specialist, whose entire time is devoted to the development of his especial branch. Advertising only and through distinctly medical channels would thus establish "protection" for specialties against the inroads of general practitioners—a form of unskilled and cheap labor fatal to the development or continuance of any specialty—and the medical man from a distance would be enabled to know with whom to consult as an authority in difficult cases requiring special skill. At present, such an inquirer is frequently deceived by the continuous "puff" every would-be-considered specialist gives himself in the medical journals and often approaches with a reverence a consultant who in fact knows possibly less about the specialty than the acknowledged general practitioner who seeks him out. It would undoubtedly be the duty of a medical editor to see to it that no medical advertisement entered his journal from a professing specialist who was not one in fact; he must, if only in his own interest, use the same careful supervision necessary to exclude any other bogus advertisement. Nor would he permit the advertiser (if any specialist showed so little sense of decency as to wish it) to recite the superiority of his personal merits over those of his competitors. But such an advertisement as appears in all our medical directories in the list of physicians, with the addition that each insertion be placed under the caption of the specialty to which such an individual exclusively devotes himself, would in our opinion be both beneficial and proper. The present defenseless condition of all specialties has invited and incurred the inrush of a multitude of medical "wreckers," who are ever on the lookout to increase their pecuniary gains. Specialties, as such, therefore are fast ceasing to exist, under the strain of competition between those who are and those who "profess," and many men, well-equipped by previous training, special talent and personal desire to devote all their time and energy in one special field, are compelled to face the alternatives of becoming "jacks-of-all-trades" or starvation. If any check could be placed upon the ruthless ravages of outsiders, we believe many men, thus debarred, whose intentions are honest and upright, would become true specialists and thus raise to a standard of accurate knowledge and dignity branches of our science which have now fallen in a state of decadence and confusion which, in the case of gynæcology at least, points to a not very far distant desuetude.

We firmly believe in the necessity of specialists in medicine, in the sense of exclusiveness, and we will gladly adopt and further any just means by which they may be protected and encouraged. If by opening our advertising columns to them such an end may be even partially attained, we will not only gladly do so but will also urge upon all our special medical contemporaries the wisdom of the same course.

It does not need the gift of prophecy to foresee, in the present trend of medical desire and practice, that the day will soon come when the hunger for advertising, induced by the unjust competition we have described, can no longer be restrained under the cloak of pretense and apparent respect for the law. When that time arrives, the profession will burst, like an irresistible deluge from the time-honored barriers of dignity and self-restraint and, in spite of county medical societies and the hopeless protests of the conservative, will vie publicly with patent-medicine men and quacks for the suffrages of the laity.

To delay this tendency, to direct and, if possible, to divert it, we suggest this method of medical advertising, at once both dignified and justifiable.——Editorial in the American Gynæcological and Obstet. Journal.

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THERAPEUTICS.

THE USE OF FORMALDEHYDE AS A DISINFECTANT.

By E. A. de SCHWEINITZ, Ph.D., M.D.
Professor Chemistry and Toxicology Medical Department, Columbian
University, Washington, D. C.

In order that a disinfectant shall be thoroughly satisfactory there are a number of conditions which must be fulfilled. In the first place, it should destroy surely and quickly the most resistant forms and spores of injurious bacteria. It must be a substance that can be easily used, non-toxic and non-destructive to mineral or vegetable matter in the concentration necessary to insure complete disinfection. Further, it should be a substance which can be applied in a gaseous condition to secure thorough contact and penetration of the objects to be disinfected. Again, it must be a substance which is stable in character, not easily decomposed, cheap, and if possible, possesses an odor which dissipates quickly, and should be a good deodorizer. To a certain degree formaldehyde possesses all of these properties and its practical use has been the subject of a number of investigations. Commercially, we find formaldehyde in the market as a 40 per cent. solution of the gas in water or alcohol under the trade names of formalin and formol. This gas can be prepared with great ease. Already in 1888 the strong antiseptic properties of formaldehyde were recognized by Law, and in 1893 Trillat published the statement that a bouillon containing 1 part of formaldehyde to 50,000 was not suitable for the growth of the anthrax germ. Aronson after making similar experiments tried the poisonous properties of the gas upon guinea pigs and found that they could live for an hour in an atmosphere rich in formaldehyde gas. During this experiment the animals were very restless, but recovered very quickly in normal air. Zuntz had already shown that the poisonous dose for rabbits after a subcutaneous injection was about 0.24 grams per kilo.

In 1893 formaldehyde was recommended for the disinfection of brushes and combs as well as for use to destroy the germs of diphtheria, tuberculosis, cholera and the like on such materials that would be injured by other disinfectants.

Another authority asserted in 1893 that after one hour in solution of a million and after $\frac{1}{4}$ hour in 1:750 anthrax and tetanus germs were destroyed. The results further showed that in the air 2.5 per cent. by volume of formalin or 1 per cent. by volume of formaldehyde gas was sufficient to destroy fresh virulent cultures of typhoid, cholera, anthrax, etc., in $\frac{1}{4}$ hour. Other experiments have shown that bandages and iodoform gauze can be kept well sterilized by placing in the jars containing these same formaliths, and it was also possible for Stahl to make carpets and cloth materials germ free by spraying them with 0.5 to 2 per cent. formalin solution for $\frac{1}{4}$ to $\frac{1}{2}$ hour, without the color of the carpet being in any way affected. In 1894 the deodorizing property of formaldehyde was explained to consist in a direct chemical combination of formalin with sulphuretted hydrogen and its products.

With Scatol a disagreeable constituent of the faces formaldehyde combines upon the addition of acid to an odorless compound.

Formalin further forms direct compounds with ammonia and ammonium bases which fact has been made use of to determine the quantity of aldehyde.

The researches of Pottevin in 1894 confirmed those of Aonson and Trillat, that a concentration of 1:20,000 was safely a retardent of sufficient strength.

As Walters has shown so far as the action of formaldehyde in gaseous form is concerned that is dependent upon the concentration of the gas the temperature and whether the articles to be disinfected are moist or dry. The killing of the germs is better accomplished when the objects are moist, not wet, and the temperature is 34°. A property which has long been well known for sulphur dioxide chlorine and bromine.

Of the practical methods of applying formaldehyde, those in which the gas is allowed to work in statu mascendi have given the best results. Several forms of lamps have been devised by which the formaldehyde is obtained by the imperfect combustion of methyl alcohol. Two simple ones have been described by Robinson and de Schweinitz. These have the advantage that the lamp can be filled with wood alcohol and placed in a room or the closed place which it is desired to disinfect. They have the disadvantage, however, that some of the alcohol undergoes complete combustion and a certain amount of carbon monoxide and dioxide are also obtained, and consequently a larger amount of alcohol is used than is necessary. Practical experiments made by Miguel, Bardet and others in disinfecting rooms by means of lamps have given very satisfactory results. The principle of the lamp is to allow the lamp flame to burn over a wire mantle of platinum or platinyed asbestos. Formaldehyde has also been recommended for the purpose of destroying the spores of smut by the action of 1:1,000,000 formalin solution.

In all experiments attention must be paid to the fact whether reference is made to the formalin 40 per cent. solution or to formaldehyde gas.

Walter has made extensive series of experiments with formalin and also with formaldehyde gas. His experiments were made first by preparing different kinds of culture media to which formalin had been added in different proportions. The results of these experiments upon anthrax spores, choler typhus and diphtheria germs showed that the proportion of 1:10,000 to 1:20,000 formalin, 1:25,000 of formaldehyde gas is sufficient to check the growth of germs. When the gas is used the staphylococcus pyogenes aureus seems to be the most resistant while strange as it may appear the spores of anthrax so difficult to kill by most germicides are very easily destroyed by the formalin. This is one of the principles of disinfection, laid down by Koch that some substances would be found which were destructive to pathogenic bacteria but were less injurious to ordinary germs. In order to prove the value of formalin on the large scale for the disinfection of clothing, Walter used a soldier's blue mixed with red color, immersed it in a culture of the pus germ so that the cloth was thoroughly saturated. Strips were then cut off, placed under a bell jar and sprayed with formalin solution varying in strength from 3 per cent. to 10 per cent. The strips were left in the bell jar for six hours. After that time tests were made by cultures from these strips, check strips being used at the same time. There was no change produced in the color of the cloth and the red was, if anything, a little brighter. The germs which had penetrated deeply into the cloth were all killed just as well as those upon the surface, which can be explained by the fact probably that the articles being surrounded with the jar the gas was forced deeply into the interstices of the cloth and killed those germs which could not be reached by the solution. The 3 per cent. solution gave just as satisfactory results as the stronger solutions.

The preliminary experiments that Walters made with the gas to prove the availability of formaldehyde for the disinfection of clothing, etc., without injury to the latter were satisfactory except with reference to the length of time required. This, however, was due to the practical details of the method as the source of the formaldehyde was either the powder placed under a bell jar or a lamp used in a room which was poorly adapted to the purpose as there were too many openings for the escape of the gas. The results of all of the investigations led to the following conclusions:

1. Formalin in concentration 1:10,000 makes the growth of anthrax, cholera, typhus, staphylococcus and diphtheria impossible.

2. In gaseous form a very great dilution is sufficient to check growth.

3. A 1 per cent. solution will kill pathogenic organisms in an hour.

4. With a 3 per cent. solution and the final addition of alcohol it is possible to make the hands germ free. Whether the hands are attacked by this method remains to be proven.

5. By spraying with formalin solution and subsequent enclosure of the articles in a closed space they can be made easily sterile.

6. Uniforms, etc., on a large scale can be disinfected without injury in 24 hours.

7. Fæces are deodorized by a 1 per cent. solution and are in 13 minutes germ free.

Formaldehyde is a useful etching and preservative material.

As compared with other disinfectants, as sublimate, acid carbolic, lysol, and the like, formalin has the advantage of being not interfered with by the albuminoid matter, does not injure the articles used. Its adoption, therefore, seems to be well recommended and to fill many requirements which are not now satisfied by existing disinfectants. Especially is this the case in disinfecting rooms, bedding, clothing, and the like. Experiments of Robinson and others have demonstrated its use in this connection.

Experiments made by Roux, Trillat and others upon the use of formaldehyde vapor for disinfecting rooms carried on at Montpellier have been very satisfactory, in that the bacilli of anthrax, tuberculosis and diphtheria have been killed within five hours by a saturated atmosphere of formaldehyde. After two days of thorough ventilation no odor remained in the room, nor were the objects which had been exposed to the action of the gas in any way injured. An objection to the use of formaldehyde has been raised because it adheres to clothing and upholstered material so firmly, but this can be removed by a careful ventilation or by the use of a dilute solution of ammonia, which readily absorbs the gas, and placed in a room where this had been used as a disinfectant would aid in a more rapid dissipation of the odor and not injure the materials. It would seem that in formalin and formaldehyde we have at hand the most useful disinfectant yet known, the application of which is a mere detail to be easily worked out in practice.

It also appears to be useful as a means of preserving foods, milk, etc., 1:10,000, but the effect upon the digestive ferments taken internally has not been thoroughly studied, though the quantity necessary to protect against bacterial growth should be too minute to produce bad results, and none have been noted, though formalin has been extensively used for preserving milk.

One liter of pure wood alcohol burned to formaldehyde gas, if there is no loss by further combustion, should give 361 liters of the formaldehyde gas.—The Charlotte Medical Journal.

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MISCELLANEOUS.

Substances Incompatible with Antipyrin.—1. Those which precipitate the antipyrin, such as phenic acid in concentrated solutions, tannin and its preparations, tincture of iodin, chlorids of mercury, etc. 2. Those which decompose antipyrin when triturated dry with it: calomel, which forms with it a toxic combination, naphthol; chloral, which forms an oleaginous liquid; sodium bicarbonate, which generates an odor of acetic acid; sodium salicylate, which also forms an oleaginous compound with it, and lastly the salts of quinin and caffein, whose solubility is increased by antipyrin.—Journal A. M. A.

PLASTIC SUBSTITUTION OF THE MAMMARY GLAND BY A LIPOMA.—By Professor Czerny (Heidberg). A woman with interstitial mastitis, with adenofibromatous formation submitted to amputation of the left breast. She had largely-developed breasts, and possessed also a lipoma, somewhat larger than a fist, situated in the right lumbar region. In order to remedy the asymmetry consequent upon the removal of the breast, she desired to have the lipoma transplanted in its place. Two finger breadths below the nipple the skin was divided by an incision with its convexity downward. The overlying skin was thus preserved and the whole gland removed through this curved incision. After thoroughly checking the hæmorrhage, the lipoma was rapidly enucleated and transplanted into the place of the breast under strict aseptic precautions, and the skin sutured with the exception of places for small drainage openings. At the end of eight days the skin-wound had entirely healed. There was no inflammatory reaction. The wound remained rather sensitive for six months. After the lapse of a year's time the tumor had neither enlarged nor contracted, and formed a rather shapely breast.—Verhand. der Deut. Gesell. f. Chir.; Annals of Surgery.

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REVIEWS.

Lectures on Appendicitis and Notes on Other Subjects. By Robert T. Morris, A.M., M.D., Fellow of the New York Academy of Medicine, etc. Second edition, revised and enlarged; with illustrations by Henry Macdonald, M.D. G. P. Putnam's Sons, New York, 27 West Twenty-third street; London, 24 Bedford street, Strand. The Knickerbocker Press, 1897.

The second edition of this excellent monograph has appeared recently. This is all the more remarkable, in view of the fact that the disease of which it treats has in such recent years for the first time received detailed separate consideration. The author's original way of putting things is nowhere better illustrated than in the first few lines of the preface, in which he says: "Eight years ago, when there was a confusion in antiseptic method of one form, I presented a little book which was accepted because it told of one way for accomplishing certain things. At the present time, while there is confusion of ideas on the subject of appendicitis, it is perhaps a favorable time for blazing one clear trail through the subject in a similar way. In the matter of proper procedures, I have due respect for methods which are different from my own, believing that in the art of surgery every surgeon is a law unto himself, and that he knows the factors of his own success." The full line of illustrations and the excellence of the same, together with the thoroughly practical way in which the author presents his facts, makes the volume a most desirable one to every operating surgeon. No one knows better than the author that every case of appendicitis is not amenable to treatment by an inch and a half incision, nor capable of recovery in ten days. At the same time, no other surgeon has done more, and few as much as the author, to demonstrate the large number of cases in which such a small incision is entirely satisfactory, as well as to demonstrate that all the available gauze in an operating room does not have to be put into an opened abdominal cavity for drainage. While the added notes in this little volume are all interesting, they bear no particular relation to the subject matter, and could with more propriety have been collected in a miscellaneous volume of their own.

The International Medical Annual and Practitioner's Index. A work of reference for medical practitioners. 1897, 15th year. New York, E. B. Treat, 241-243 West Twenty-third street. Chicago, 199 Clark street. Price, $2.75.

The present volume is uniform in a large measure with those which have appeared for fifteen consecutive years, and to the old subscribers it is unnecessary to say a word in regard to the value of the facts or the compact way in which they are presented. This year the illustrations, it seems to us, are a little more full than usual, and the volume itself is perhaps a trifle larger. The alphabetical arrangement of diseases and treatment makes the book one which can be consulted easily and rapidly. The very able list of contributors to this work always insures a high degree of value to the articles, as well as a warm reception by the profession.

Lectures on the Treatment of Fibroid Tumors of the Uterus, Medical Electricity and Surgery. By Franklin H. Martin, M.D., Professor of Gynæcology, Post-Graduate Medical School of Chicago, etc. Author of Electricity in Obstetrics and Gynæcology. Chicago, The W. T. Keener Co., 1897.

This little volume of 175 pages, well illustrated, gives the details regarding fibroids of the uterus in a way that cannot be misunderstood. The method pursued is to give the various treatments which have given good results, and to show the application of such treatment to individual cases in the author's experience. Naturally, the operative side of the question receives the fullest consideration, and, as a matter of fact, in view of the excellent results which are to-day being obtained in this line of work, there is very little excuse for not using an operative procedure in the treatment of a rapidly growing fibroid. The immense fibroids of years gone by ought to become rarer and rarer, as well as the very troublesome conditions which constantly arise in the course of their growth.

While the illustrations are, from an artistic standpoint, not perhaps as good as many that appear, from a practical standpoint they are exceedingly useful as well as helpful.

Principles or Guides for a Better Selection or Classification of Consumptives Amenable to High Altitude Treatment and to the Selection of Patients Who May be More Successfully Treated in the Environment to which They were Accustomed Previous to Their Illness. By A. Edgar Tussey, M.D., Adjunct-Professor of Diseases of the Chest, in the Philadelphia Polyclinic and School for Graduates in Medicine, etc. Philadelphia, P. Blakiston Son & Co., 1012 Walnut street, 1896.

NEWS AND NOTES.

BULLETIN OF THE AMERICAN ACADEMY OF MEDICINE.
THE PHILADELPHIA MEETINGS.

I.

AMERICAN ACADEMY OF MEDICINE.

The twenty-second annual meeting of the American Academy of Medicine will be held in Parlor "C" of the Continental Hotel, Philadelphia, on Saturday and Monday, May 29 and 31, 1897.

FIRST DAY.

The meeting will be called to order at 2 P. M., when the Academy will hold an Executive Session, open to its membership only.

ORDER OF BUSINESS.

Reading of the minutes of the last annual meeting.
Report of the Council.
Election of Fellows.
Appointment of a Committee on Nominations.
Treasurer's report.
Action on the proposed amendments to the constitution.
New business.

At the conclusion of the Executive Session, approximately at four o'clock, an Open Session will be held for the reading of

PROVISIONAL PROGRAM.

Only those papers whose titles have been furnished are included in this list; several others have been partially promised and it is hoped that these will be added to the completed program to be presented at the meeting.

THE ASSOCIATED DUTIES OF THE PHYSICIAN.

I. "The True Principle on which the Medical Profession Should Be Associated and the Character of the Resulting Organization." Leartus Connor, M.D., of Detroit.

II. "Physicians Mutual Aid Societies." John B. Roberts, M.D., of Philadelphia.

III. "Quid pro Quo—Present and Future." C. C. Bombaugh, M.D., Baltimore, Md.

IV. "The Relation of the Physician to the Public Press." Solomon Solis Cohen, M.D., of Philadelphia.

V. "Some Relations of Author, Publisher, Editor and Profession." George M. Gould, M.D., of Philadelphia.

VI. "Medical Reviews." Walter L. Pyle, M.D., of Philadelphia.

VII. "The Influence of a Liberal Education with Reference to Medical Ethics." Elmer Lee, M.D., of Chicago.

VIII. "Hospital Abuse." W. L. Estes, M.D., South Bethlehem, Pa.

IX. "Result of a Year's Endeavor to Lessen the Dispensary Abuse in the Rhode Island Hospital, Providence, R. I." F. T. Rogers, M.D., of Providence.

X. "Are Physicians Up to Date? A Sociologic Inquiry." Charles McIntire, M.D., of Easton, Pa.

The Academy will take recess at about 6.00 and reconvene at 8.00 in Open Session.

XI. The President's Annual Address.—J. C. Wilson, M.D., of Philadelphia.

At the conclusion of the address, if it be the pleasure of the Academy, the papers not reached in the afternoon will be read.

SECOND DAY.

Executive Session, 10 A. M.
Reports of Committees.
Election of Fellows.
Election of Officers.
Unfinished Business.
New Business.

Open Session, 11.00 A. M. Reading of papers resumed.

XII. "The Relation of Alcohol to Preventive Medicine." J. W. Grosvenor, M.D., of Buffalo.

XIII. "The Truth about Calomel." Everett Flood, M.D., of Baldwinsville, Mass.

XIV. "The Great Physician of the Revolution: A Doctor sans peur et sans reproche." A. L. Gihon, M.D., Medical Director, U. S. N., retired.

XV. "Where Shall We Put Up the Bars? A Plea for Preliminary Education." A. L. Benedict, of Buffalo.

DISCUSSION: "THE RELATION OF THE COLLEGE TO THE MEDICAL SCHOOL."

XVI. "The Side of the Medical School." Bayard Holmes, M.D., College of Physicians and Surgeons, Chicago, Secretary of the Association of American Medical Colleges.

XVII. "The Side of the College." Ethelbert D. Warfield, LL.D., President, Lafayette College, Easton, Pa.

XVIII. "The Side of the University." William Pepper, M.D., LL.D., ex-Provost, University of Pennsylvania, Philadelphia.

The Committee on Program will ask that thirty minutes be given to each of the speakers opening this discussion, and, at their conclusion, that visitors be invited to take part in the discussion, each speaker being limited to five minutes according to the rule. The discussion will be assigned to the first place after the recess for lunch.

At the conclusion of this discussion, the reading of any unread papers will be in order, after which there will be a short executive session.

Reports of Committees.
Unfinished Business.
Induction of President-Elect.
Appointments for 1897-8.
New Business.

The Reunion Session will be held at the Continental Hotel on Monday, May 31, at 8.00 P. M.

By a standing rule of the Academy, the price for the supper is limited to two dollars a plate. Fellows are at entire liberty to invite their friends to enjoy with them the Academy's annual social function. The meetings during the past years have been pleasant and the post-prandial program of interest. It greatly aids the committee in every way to know in advance the probable attendance upon the reunion session. Those who may be thinking about attending are requested to send their names either to the chairman of the Committee of Arrangements, the President, or the Secretary. The price for the ticket can be paid to either of them at any time.

NOTE. The time limit for papers read before the Academy is twenty minutes; discussion to five minutes for each speaker, excepting in closing, when ten minutes are given.

The Trunk Line Association, with their usual courtesy to the

Academy, extend the time of the concession granted to the American Medical Association to include the earlier date of the Academy. Fellows desiring to avail themselves of this will please—(1) Pay full fare to Philadelphia and secure a certificate of that fact from the agent selling the ticket. (2) This certificate must be endorsed at Philadelphia by Dr. W. B. Atkinson, Secretary of the American Medical Association, and countersigned by an agent of the Trunk Line Association. (3) When so endorsed and countersigned the presentation of the certificate will secure you a return ticket by the *same route* at one-third fare.

The Council will meet on Saturday morning, when the applications for Fellowship will be acted upon. Kindly forward any application you may have to the Secretary as soon as convenient.

The American Laryngological, Rhinological and Otological Society will hold its third annual meeting at Washington, D. C., May 1 and 3, 1897, in Columbian University, corner Fifteenth and H streets. Franck Hyatt, president; Robert C. Myles, secretary and treasurer; vice-presidents, Frederick L. Jack, John S. Mabon, James E. Logan, William Scheppegrell. The profession is cordially invited to attend.

Chicago, Ill., April 3, 1897.

Editor of Gaillard's Medical Journal, New York, N. Y.

Dear Doctor:—We believe that the comfort of those American physicians who, with their relatives and friends, intend visiting Moscow during the meeting, next August, of the twelfth International Medical Congress, will be greatly enhanced and the journey itself rendered more interesting and pleasant, as well as more economical, by proceeding as one party, instead of traveling singly or in small groups. Acting on this belief we have secured considerable reductions in the ordinary steamship, railway and hotel rates, and have succeeded in arranging the enclosed itinerary with the well-known tourists' agents, Messrs. Cook & Sons. They assure us that the charges quoted are as low as is compatible with first-class service throughout the trip.

An educated conductor will accompany each section of the party, and the journey may be broken at almost any point, to meet emergencies.

Will you assist in giving America her proper representation at the Congress by publishing as full a notice of the excursion as you can, and, if possible, by joining it yourself ?

Very truly yours,

N. Senn, Casey A. Wood, Harold N. Moyer, Eugene S. Talbot, D. R. Brower, J. B. Murphy, D. A. K. Steele, B. T. Whitmore.

GAILLARD'S MEDICAL JOURNAL.

A MONTHLY JOURNAL OF MEDICINE AND SURGERY

Scientia et Veritas Sine Timore.

M. E. GAILLARD, (PUBLISHER.)

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All business communications should be addressed to Gaillard's Medical Journal, P. O. Box 1124, New York. All editorial communications to Carter S. Cole, M.D., 101 W. 74th St., New York.

Articles for publication will be received with the understanding that they are contributed exclusively to this journal. Reprints will be furnished at actual cost, orders for which should accompany the manuscript. Authors of accepted articles may receive twelve copies of the issue in which they are published. Necessary illustrations will be furnished without expense to authors when suitable drawings or photographs are furnished.

EDITORIALS.

THE DEATH OF DR. ERNEST BRAND.

Although the chief object of the doctor's mission is to cure disease, therapeutics is a branch of medicine which seems to lag far behind all other branches in the march of progress.

It is, therefore, a sad office to record the departure from this life of a man who has made the most substantial additions to life-saving therapeutics in modern times.

The treatment of typhoid fever has passed through many phases, and is still the subject of earnest consideration, whenever practical physicians meet to discuss the chief problem of their daily lives. The expectant plan which resulted from the swinging of the pendulum in the opposite direction, after bloodletting and other depletive measures, held sway until the antipyretic theory pointed to high temperature as the chief lithol element. Then came Liebermeister's frequent cold baths and heroic doses of quinine, which improved the mortality statistics and elicited psalms of praise for the originator of this method. Then came those real antithermic agents, derived from coal tar, which bid fair to revolutionize typhoid fever management. If high temperature was the lithol element, we had a key to its suppression in these powerful antipyretics. How sadly we were disappointed is recorded in the history-book of every hospital and graven in letters of sorrow in many a household. Then came the antiparasitic theory of treatment, in which we are now floundering, and from which we are likely to emerge wiser men. In the meantime one method survives and meets all the demands of the most advanced science—the methodical bath treatment. This was inaugurated by Ernest Brand in the sixties, and stands to-day—thanks to his bold, unswerving, ceaseless advocacy—unshaken, because it is clad in the panoply of clinical results of undisputable character, which defy the skeptic and scoffer alike.

That the Brand method has greatly diminished the mortality of typhoid fever was attested many years ago by Gottstadt, the director of the Imperial Statistical Bureau in Berlin, whose statement led the surgeon-general to recommend its adoption in the German Army.

It was again attested last fall by Prof. Gerhardt, when, in opening his course of lectures in the Berlin University, on "Prognosis," he dwelt upon the change wrought by Brand's method in the prognosis of typhoid fever, which was thereby reduced to one-fourth of its former mortality. Although the value of this systematic method is not yet fully recognized, chiefly because of the difficulty attending its execution and its seeming heroic character, the student of clinical medicine discerns clearly the great boon which Ernest Brand has conferred upon suffering mankind by laboring with heroic self-abnegation for the recognition of his simple and effective method.

We are called upon to mourn the death of this noble man, who, though not clothed in the paraphernalia belonging to a professorship, sent out from his humble private practice, the glad tidings of a treatment which, if adopted early, as he has insisted, renders the physician the master instead of the watchful guardian and slave of a disease, which has hitherto tantalized him by its sudden turns and appalled him by its fatal complications, against which he has felt himself powerless to contend. Brand has enabled us to prevent these complications; he has smoothed the path of patient and physician alike. May his memory be blessed, and serve to incite us to humble emulation !

ERRORS IN THE MANAGEMENT OF INFANTS.

A recent paper before the Hufeland Society of Berlin, for an abstract of which we are indebted to our excellent contemporary, the *Deutsche Medicinal Zeitung* (March 15, 1897), emphasizes the excesses committed in the management of infants.

Dr. Neumann presented some views to the society which, though somewhat iconoclastic, bear so much impress of truth and practical value, that our readers may profit by a brief reference to them.

The bathing of the newly-born is regarded as unnecessary, because the child is issued into the world in a practically aseptic condition. Indeed, it is much cleaner than would seem, the veanix caseosa being an excellent protective which is not liable to decomposition, and which we should not be so anxious to remove as is usually done. The water in which the infant is bathed is regarded, and, we believe, justly so, as a frequent source of infections of the navel, which is usually handled too carelessly by the nurse. It would be better to use more caution in preventing infection of the navel by less manipulation than to be over-solicitous in dressing it with disinfectants. Neumann regards it as advisable to defer all bathing of the infant until after the cord has dropped off. The first bath may be retained, provided the infant is cleansed with soap and a clean sponge, before being put into it. Neumann quotes with approval the practice adopted in the Woman's Hospital of Philadelphia, where immediately after delivery of the head the infant's eyes are washed with an antiseptic solution, and the cord is not severed until the placenta is spontaneously delivered; when the mother is cared for and all the flow is stopped, the cord is cut afterward, and neither tied nor dressed; the child is wrapped up warm, and receives its first bath only after the lapse of twenty-four hours. If the first bath is insisted upon, subsequent baths should be omitted; the navel cut quite short after being tied well, and dressed with a cotton pledget, which should be removed on the third day and then every second day, until it drops off. Then the infant may be bathed and swathed daily. This is the method pursued in the maternity at Buda-Pesth, in which out of 230 newly-born, only 3.5 per cent. presented a febrile cord infection, and 11.9 per cent. had fever at all, while formerly the percentages were 22 and 45 respectively. Bathing the newly-born may be injurious, also, because it cools the body too rapidly. Infants that are not bathed are more prone to gain weight than those who are bathed.

In bathing the eyes of infants, it is a too common practice of nurses to cleanse the conjunctiva, which is not necessary unless a purulent discharge exists and may cause abrasion. Another excess is committed in cleansing the mouths of newly-born infants, which usually are clean enough. The practice of covering a finger with a cloth and scrubbing the delicate lining membrane of the cheeks and tongue and gums of the little one, cannot be too severely condemned. Stanolitis is frequently the result of abrasions thus produced; aphthæ result, which are again scrubbed, and which would heal more readily if left alone.

Cutting the frenum linguæ and scarifying the gums are slowly going out of fashion. Careful observation teaches the judicious practitioner the adsurdity of cutting the gums, and enables him to withstand the entreaties of anxious parents and friends to relieve the child by this barbarous procedure. Who has not observed how quickly such incisions heal and leave a scar, which presents the same obstruction to the passage of the tooth. Modern methods of examinations of sick infants have become so thorough that other causes than "difficult teething" may be readily discovered, to which the infant's illness may be traced. It is somewhat surprising to find, therefore, that Neumann regarded it necessary to inveigh against "cutting gums" in this enlightened era of pædiatrics.

The young practitioner cannot be too guarded in watching the infant's first bath, especially providing against the use of any but boiled water, which should be applied with clean, fresh absorbent cotton and never with a sponge, since the latter is prone to be used for other purposes. The warning against injuring the lining membrane of the mouth is quite apropos; we do not remember its previous publication. Unless some of the fluids issuing with the child have entered its mouth, the interior of the latter should be scrupulously avoided by the well-meaning but officious nurse.

A TRIBUTE TO HEROISM.

We cannot pass without comment the recent tribute to the memory of two valiant and able physicians who sacrificed their lives during the yellow fever epidemic of 1877 in Fernandina, Fla. A stained glass window has just been placed in memory of them in St. Peter's Church there, and never was such a tribute more well deserved at the hands of a grateful community. The physicians were Drs. Francis Preston Welford and James Carmichael Herndon, both of whom volunteered their services and moved to Fernandina to fight the epidemic. Dr. Herndon had already a few years previously demonstrated his courage and ability in a similar epidemic. The window was given by Dr. J. Baxter Upham, and if the people had added a monument they would not have lavished too much upon the memory of these fearless and able physicians. It is a rather remarkable fact that monuments are so often erected to men who have done so little in comparison to the great work of some of the great physicians, while the tributes to men in our own profession in this especial line are so few as to be capable of enumeration in a very few lines. This may be due to the fact that the community remark the physician as simply doing his ordinary work, and yet however much he might be blamed for shirking or trying to escape his regular line of duty, when, as has so often been the case, he volunteers to go into the very midst of a death-dealing epidemic and contribute to the relief and recovery of the sick and suffering, he cannot be regarded in any other light than a hero.

GAILLARD'S MEDICAL JOURNAL.

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ORIGINAL ARTICLES.

PATHOLOGY AND TREATMENT OF INTERNAL HAEMORRHOIDS BY LIGATURE, WITH CASE.*

By WM. H. PARKER, M.D., Richmond, Va.

Professor of Genito-Urinary and Rectal Surgery in Summer School of
Medicine, University College of Medicine, Ex-resident Surgeon
Richmond City Hospital, Etc.

The disease commonly called piles is the most common of all human ailments, except the ordinary "cold." Yet there are unfortunately few diseases in which, as in piles, the surgeon is able to assure the patient in every case that he can positively be cured. Piles have their origin in the over-distended and varicose hæmorrhoid vessels, principally the veins modified by the mechanical violence to which their position exposes them. The vessels are very liable to become varicose—first from the fact that valves are absent in the veins; second, because of the erect position; and third, because of the peculiar office of the rectum.

The veins immediately around the verge of the anus form a continuous network by anastomosis with the hæmorrhoidal plexus of the rectum within. These veins anastomose even in the substance of the sphincter muscle. Therefore from the character of the veins in which they take their origin, there is no distinction between internal and external piles.

Now these veins have no valves, and, consequently, whenever the portal circulation is sluggish or an abdominal tumor, ovarian, etc., or pregnancy exists, we have a stagnation of the blood and a dilated and varicose condition of the hæmorrhoidal veins—a condition similar to varicose veins of the lower extremities or a varicocele.

Owing to the constant erect position is it a matter of surprise that we should find this varicose enlargement with thickened walls and pouch-like dilatations?

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*Read before the Richmond Academy of Medicine and Surgery, April 13, 1897.

Owing to various causes, as constipation, straining at stool, etc., this stagnation and congestion leads to an exudation, and the connective tissue surrounding the veins becomes infiltrated, and we have the morbid anatomy of the tumor. It is a veritable tumor—one that can be seen, is well defined and can be handled, is firm to the touch, and grows by plastic infiltration.

Internal hæmorrhoids are of a much more serious nature in so far as the health of the patient is concerned than external hæmorrhoids.

French surgeons have not accepted as true the causes of piles, ordinarily spoken of, but have diligently sought for a true predisposing cause in the anatomy and physiology of the rectum.

By numerous dissections, Prof. Verneuil thinks he has discovered that cause in the peculiar distribution of the veins and the course they take in the coat of the rectum a few inches above the anus. He considers that the superior hæmorrhoidal veins only communicate with the portal system and solely form the internal piles; external piles being formed by the inferior and middle hæmorrhoidal vessels which connect with the general venous system. That the superior hæmorrhoidal vessels commence at the upper border of the external sphincter and pass up under the mucous membrane to a certain height and perforate the wall of the gut. Where they perforate the walls of the rectum, he claims that they pass through veritable muscular buttons, which have the power of contracting and causing stasis of the blood in the vessels.

Verneuil affirms they are not only the passive but the active causes of hæmorrhoids. The practical outcome from the above anatomy and physiology by the French author is very important, viz.: That for the cure of the great majority of internal piles, nothing is required but the gentle and thorough dilatation of the internal and external sphincter muscles. The point chiefly insisted upon is that the dilatation should be thorough and include both sphincters. Allingham shows the fallacy of this method of treatment—first, from the fact that the absolute separation of the portal and general venous circulation cannot be true; second, from the fact of Verneuil having injected the superior hæmorrhoidal veins from the portal, thus forcing the injection in a direction opposite to the natural way; third, because he makes no allusion to the part that the arteries take in the formation of a pile; and, lastly, the fact that the muscular buttons would really act the part of valves to support the column of blood to the liver, and in place of causing stasis, prevent it by opposing regurgitation in congestion of the liver.

The medical treatment of piles, I think, does not receive the recognition it deserves in the works on rectal diseases. Indeed in some works, as Allingham, it receives no mention at all. Matthews, in his work, says he does not believe that hæmorrhoidal diseases can be cured in any other way than by operative procedure; yet Thomas, in the London Lancet, formulates a set of rules for the medical treatment of piles, claiming that a strict observance of them will cure the great majority of cases which have now to be operated on. He refers to both varieties, but more particularly to the internal.

Dr. T. Lauder Brunton, in the British Medical Journal, on the treatment of piles, says: First, keep the liver in a condition to maintain a free supply of blood through it. For this purpose insist on moderation in food and stimulants. Occasional small mercurial purgatives, followed by a mild saline, alum in small doses and castor oil do good. Exercise is useful in keeping the liver free, but this exercise must be of a certain kind. Both blood and bile have a tendency to stagnate in the liver; but this stagnation is lessened by the liver being rhythmically squeezed between the diaphragm and abdominal muscle. Brisk horseback exercise, either trotting or cantering, or touching the toes with the fingers, the knees being kept straight, is excellent exercise. A regular action of the bowels is of the utmost importance. The best time ordinarily for emptying the bowels is after breakfast, but if the piles have a tendency to come down, it is better to empty them every night before going to bed. The patient should take with him a bottle of hamamelis and some prepared sheep's wool. A small pledget of the wool should be inserted sufficiently to be grasped by the sphincters. This acts locally as a support to the piles, the hamamelis being healing and stopping hæmorrhages quickly.

The treatment of piles by the ligature is a time-honored surgical procedure.

It owes its present prominence to Allingham, and is often described as his operation. In the way now generally performed, the name is correct, though the treatment by ligature is very old. In the opinion of Allingham, of England, Wyeth, Matthews and others, in this country, it is the safest, easiest and best operation.

The following case will illustrate the method:

Mr. B., white, aged 35, consulted me in October, 1896. Had been a sufferer with piles for years. In January of '96 had been operated on by injection of carbolic acid, only to leave him worse off than before. He complained of severe pain in his rectum all the time, with protrusion of a large mass whenever he had an action. On examination, a large mass protruded on bearing down, consisting of four large vascular tumors. Patient was weak and anæmic from repeated losses of blood at each defecation. He was told that an operation would cure him, to which he consented.

Preparatory treatment consisted of 2 C. C. pills followed by a dose of epsom salts the next morning and repeated again the following morning, with an enema before the operation. Chloroform was administered and patient placed on his back with legs drawn up and held by an assistant. The sphincter was then thoroughly dilated with my thumbs and the bowel everted. Each tumor was seized with a pair of forceps and pulled down forcibly. A curved needle with a double thread was run through the base and clipped. The mucous and muscular coats were cut around the base and ligature tied as tightly as could be drawn. In this way there is no pain after the operation. The piles were then clipped off, leaving sufficient base to hold the ligature. By clipping the base, you will hasten the slough and your patient makes a quicker recovery. Each tumor was treated in the same way. The rectum was then irrigated with a solution of bichloride 1-4,000, stumps replaced and a packing of gauze inserted. Dressings were retained by a T bandage and patient put to bed and given a hypodermic of morphine and atropine.

That evening he had retention of urine, which was relieved by a soft catheter, and patient spent a comfortable night.

On third morning bowels were moved by enema of warm water without any pain or protrusion and bowel irrigated with an antiseptic solution. Patient was ordered to take wine-glass of Apenta water every morning, which acted admirably, and the following prescription given:

℞ Hydrarg. Chlor. Corros......................gr. j
Tr. Gentian Comp.........................
Tr. Cinchonae ....a af ℥ i j
M. Sig. teaspoonful in water 3 times a day.

Bowels moved every day, ligature came away from 7 to 10 days, and patient was discharged as cured in two weeks.

SOME OF THE EVILS THAT THREATEN THE MEDICAL PROFESSION.*

By W. S. BEAZLEY, M.D., Richmond, Va.,
Demonstrator of Physiology, Medical College of Virginia, etc.

A few meetings ago, Dr. Hodges in his most excellent paper, on some of the socio-medical problems which confronted us as professional men, aroused the members of the Academy to action. I wish to add to what he has already said.

In these days, when competition is so great, men are offering their services almost without money and without price. The same competition has found its way into the medical profession, and we find its followers offering their services for a small consideration.

Before mentioning some of the evils which threaten to bring our profession into disrepute, I will make two assertions which can hardly be contradicted. First. There is no class of men in a position to be as independent as the men of our profession. Second. No class of men do more charity work than do physicians.

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*Read before the Richmond Academy of Medicine and Surgery, May 11, 1897.

My first proposition, that physicians can be the most independent of all men, will appear conclusive if we consider the appeals made to us by the maimed, the halt, and the blind. Medical attention is a necessity, and who can render these services save those who have unlocked the mysteries of the human system and studied diseases contained therein? People may, and often do without treatment of a sin-sick soul; but when suffering flesh is made to cry out in agony, then the medical man is sought for relief, and he is expected to respond, even though he fails to receive money or thanks for his services.

Notwithstanding this age of educational advantages, scientific research and progress in lines of business, we may pause, survey the field, and ask the question, whether or not our profession is advancing, or whether we are being brought into disrepute?

In order to answer such questions, there are certain evils to be considered. The first to which I will call attention is the spirit of jealousy existing among men who should be foremost in the defense of ethics. This evil has invaded our city, and entered the hearts of many of us. Jealousy and uniformity of action are incompatible. With this fact staring us in the face, what is to become of the true worth of our calling?

With uniformity of action, our profession would not only occupy a high place in the estimation of the laity, but the practice of it would be worth to physicians of Richmond several times as much financially as it is now. How often do we answer calls to families only to find out that a dozen or more physicians have given them up because they do not pay. How often, in the still hour of the night, do we arise, shake off the hold of "nature's sweet restorer," to relieve a patient, and hear later, through some of the neighbors, that he doesn't pay doctor's bills? Under the present system of things, some people rarely if ever pay a doctor's bill, for where "patience ceases to be a virtue," and the doctor urges his claim, he is discharged and another called in. Of course, those whose hearts are willing, but whose flesh is weak, are not included in the list of those who delight in seeing our mental faculties burn with anxiety for their suffering flesh without compensation.

It is a sad fact that some people are not even grateful for services rendered. This may be accounted for by the fact that gratitude, being a debt, receives the same treatment as all other debts.

The obligations of patient and physician are mutual, and the physician has the right to expect from his patient the consideration of his health and comfort, and the prompt payment for services rendered.

We will all agree that in proportion to the amount of work we are more poorly paid than any other class of people.

That there is a cause for this state of affairs, all will agree. That we are responsible for the trouble, some may question. Let us see how far we are responsible. Mr. A. has his druggist to 'phone for Dr. B., and Dr. B. refuses because he has not been paid for previous visits. He then calls upon a physician who does not know him, but who, after a long service of faithful work, knows him to his sorrow. When the second physician becomes disgusted and quits, another one is likewise imposed upon, and so on down the list, until the man has lived a lifetime without paying a doctor's bill. A certain man was known to make the brag to a collector that he didn't pay doctor's bills. He sent for me, thinking I was innocent as to his fraudulent methods. I refused to attend him, whereupon he secured another physician. The last I heard of him he was changing physicians, and the Lord only knows when he will stop. I might enumerate other cases, but all of us know too well that such a state of affairs exist.

Now, if we agree that we can better the condition of affairs, we must also agree that we are largely responsible for non-appreciation and non-payment of bills by a large number of people.

What is the matter? I might sum it up in these words: Lack of Unity. Until we realize that we are in the profession of medicine, and defenders of each other, there will be discord in the ranks. The dead-beats we have with us, but all of us don't know them. They should be introduced to every physician in Richmond. Let them be known to all our medical brethren, and practice will be worth double as much as it is. In this day, when a great financial crisis is upon us, some of us look, with hands off, at questions of common interest, for fear of losing a few dollars. Selfishness makes us narrow, and we fail to accomplish the purpose for which we were intended.

There is another evil which is threatening our profession, and that is the prescribing of patent and proprietary remedies. I wish to submit three propositions, and try to prove them to you.

1st. The prescribing of such remedies is unscientific.

2nd. It is causing the general public to prescribe for themselves.

3rd. It is increasing the number of manufacturing chemists, and thereby allowing them the privilege of dictating to us as to treatment of disease.

To prescribe a remedy about which we know nothing is unscientific and unsatisfactory to the physician. All of the patent medicines, and many of the proprietary remedies, are secret preparations. I will mention three remedies, which will illustrate my point. These are ammonol, pyroctin, and acetifas. If the formula for ammonol has been given, I have never seen it. Pyroctin is said to be a febrifuge anodyne, and antipyretic. What is the nature of pyroctin? Does it grow on trees, or come out of the ground? We are told that acetifas is acetifas and citrate of caffein. After we have read the formula, we may pause and ask, What is acetifas? We teach our men at college to formulate their own prescriptions. Let us practice what we preach.

I have been told that some of our medical brethren have looked with favor upon castoria, while a few have given Mrs. Winslow's soothing syrup consideration.

In regard to the second point, that it teaches the laity to prescribe for themselves, we have only to listen to our patients as they tell us Dr. So-and-So prescribes Henry's tri-iodides and chlorides. Some people buy these ready-prepared mixtures rather than send for a physician because of the fact of their hearing that physicians prescribe them.

As to the third point, that such prescribing is increasing the number of manufacturing chemists, and thereby allowing them to dictate to us as to treatment of disease—we have only to look on our tables, bookcases, and mantelpieces, and behold a multitude of worthless remedies. Representatives of these various remedies tell us all about them, the indications for their use, and the surety of their action. Sometimes we take their advice.

Our position should be that of leaders, and not followers. We should be the teachers in this medical world, and elevate rather than lower our profession. We may go back to the days of the pioneers of our noble calling, and ask ourselves if we hold the honor and the dignity of our profession as sacred as they did?

Another evil found among us is our willingness to give testimonials. We not only prescribe patent and proprietary remedies, but we give testimonials as to their merits. These testimonials often go before the public, and, as a result, they patronize these remedies rather than the doctor. May the time soon come when physicians will say to the manufacturers, "We are tired of having you tell us what we want and ought to use in our practice. We will prescribe what we need and get our local druggists to fill the prescriptions."

Of course there are preparations whose formulas are given, and which have proven beneficial in the treatment of disease. Cruel, indeed, would we be to withhold the means of alleviating the pangs of human suffering; but in treating our patients, let us know what we are giving.

As physicians, we ought to be practical men. In administering remedies, we should have a definite purpose in view, and use those from which, from our knowledge of the physiological action of drugs, we can expect results.

To experiment with the secret combinations of remedies, now being sent out to the physicians and the public, is unwise.

The man who fires a load of shot into a covey of birds, hoping that some of them may get a bird, does not deserve as much credit as the man who, taking aim at one particular bird, brings him down.

And now, Mr. President, in closing, let me urge the members of this Academy to defend not only their individual interests, but the great principles upon which rests our profession. Narrow indeed is the world to the man whose ideas are confined to the building up of himself; but he whose life is thrown into the life of others lives in closer touch with the spirit of progress which has blessed our nation.

Some may say to young men, "Our profession is overcrowded; stay out." I would answer them by saying there is room for good men in every calling of life; come on! Let us push onward and upward in the medical world, and always remember that it is a good thing for "brethren to dwell together in unity."

2600 East Broad Street.

SELECTIONS.

SOME IMPORTANT FACTS ABOUT CHLOROFORM.*

By H. A. HARE, M.D.,

Professor of Therapeutics and Materia Medica in the Jefferson Medical College of Philadelphia.

The effects of chloroform upon the vital functions has been for many years a subject of hot dispute between various surgeons and pharmacologists. On the one hand has been the school originally led by Syme, of Edinburgh, which has asserted that death or danger from chloroform lay in failure of the respiration and that this was the function to be watched while chloroform was being used; on the other hand the so-called London school has asserted that death arose from cardiac failure and that the pulse was the thing to be watched during the use of chloroform. The number of clinical and laboratory researches which have been carried out to determine which of these opinions was correct has been very great, both in England and in America, but it has only been within the last ten years that the greatest and most competent studies have been undertaken, of which the best known is the series made through the munificence of His Highness the Nizam of Hyderabad, in India, and known as the "Report of the Hyderabad Chloroform Commissions Nos. 1 and 2." The conclusions of the first commission having been criticised adversely because it asserted that the cause of death was respiratory failure, a second commission was formed, and under a grant of $5,000 from the Nizam, Dr. T. Lauder Brunton, of London, was sent out to direct the second investigation. Although a teacher of the theory that death from chloroform is due to cardiac failure, this gentleman returned to England converted to the idea that the cause of most of the fatal accidents under this drug is respiratory failure, and the conclusions of Commission No. 2 were once more criticised by numerous clinicians and pharmacologists in England and America. About this time the author and H. C. Wood published a paper in which they expressed their belief in the powerful depressing effects produced by chloroform upon the heart; and McWilliams, of Glasgow, proved by experiment that chloroform without doubt causes cardiac dilatation and enfeeblement. Shortly after this the author of this article was asked by the Government of His Highness the Nizam, through Colonel Elward Lawrie, the Residency Surgeon, to carry out a series of studies designed if possible to reconcile the contradictory results reached by the Hyderabad Commissions and other investigators, and simultaneously Gaskell and Shore in Cambridge University, England, were asked to do likewise. The result has been to confirm in every way the results of all previous studies in one respect, namely, that the primary action of chloroform, on the vital functions of circulation and respiration, is greatly to depress the vaso-motor system, thereby causing an extraordinary fall of blood pressure. Gaskell and Shore, to be sure, assert that anæsthesia can be produced by chloroform without causing this fall, but the author has never been able to do so. They also believe that the fall is chiefly a result of cardiac failure. With this view the author dissents, because their experiments upon which these assertions are based were too complex to give positive results; and second, because of his own studies and those of others show the vaso-motor system to be depressed. There is, of course, some cardiac enfeeblement and dilatation, which adds to the fall of pressure.

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* A paper read before the College of Physicians of Philadelphia, January 6, 1897.

As with other discussions in medicine, the truth of the question as to whether chloroform causes death by respiratory failure or cardiac failure lies as it were half-way between the two antagonistic forces; and, further than this, the somewhat startling statement may be made that it is not directly due, in the majority of cases, to either of these causes. On the contrary, the cause of death from chloroform is usually vaso-motor depression, whereby the arterioles allow the blood to pass too freely into the great blood-vessel areas which are found in the capillaries and veins, and as a result the man is suddenly bled into his own vessels as effectually as if into a bowl. When it is remembered that the capillary network of the body will, with the relaxed veins, hold many times the normal quantity of blood, and when it is remembered that we can inject salt solutions into the vessels to the extent of several times the normal quantity of blood without raising the blood pressure, it at once becomes evident that the complete vascular relaxation caused by chloroform results in failure of all the vital functions, not because the drug has paralyzed the heart or respiratory center, but because these parts are deprived of blood by its stagnation in the widely dilated capillaries and abdominal veins. Recent studies by Leonard Hill on "The Physiology and Pathology of the Cerebral Circulation" showed that this was the case, for he asserts that when the blood was no longer flowing to the respiratory centers the heart was still beating, because its coronary arteries, being lower down, were more easily supplied by the small blood stream received by the heart from the veins. These studies are proved by the experiments of myself and my assistant, Dr. Thornton (Therapeutic Gazette, October, 1893), by every tracing of the Hyderabad Commissions, and all other tracings we have ever seen. We may conclude, therefore, that while chloroform without doubt acts as a powerful depressant poison to the respiratory center and the heart, in the same manner as it paralyzes all living protoplasm when applied in excess, that when properly given by inhalation it produces a death equivalent to that resulting from hæmorrhage, which is a failure of the respiration not so much from a direct depression of the respiratory center as from its starvation of blood; and while the tendency of the drug is to depress and dilate the heart, just as it dilates the vessels of which the heart is merely a highly specialized part, the failure in the pulse rests upon vasomotor palsy, the patient becoming pulseless because the heart has not any blood to pump.

Let us see what evidence supports this view: First, we have the laboratory tracings of many independent investigators extending over many years and made in all parts of the world, all of which show a fall of blood pressure. Among these may be named Bowditch and Minot, of Boston; Coats, H. C. Wood, Gaskell and Shore, the Hyderabad Chloroform Commissions, the studies of Wood and myself in 1889 and 1890, and of myself and Thornton in 1892 and 1893. They are confirmed by Hill, who has seen the abdominal vessels engorged with blood under chloroform, the medulla almost bloodless and the heart still pumping though respiration had ceased. They are confirmed by my own experiments, in which I proved that even after the respiration had stopped and the carotid was empty, and the dog apparently dead, he could be resuscitated by visceral compression and artificial respiration, and by inversion whereby the blood left the dilated abdominal veins for the heart and brain. Again, if a needle was inserted through the chest wall the heart was found to be beating for the needle moved to and fro; and finally if the chest was opened the heart could still be found beating feebly—dilated, it is true, but beating.

So much for the laboratory evidence. What have we in clinical evidence? Equally positive proofs of vaso-motor palsy, and none of death being purely cardiac or respiratory. For years Chisholm, of Baltimore, and later Howard Kelly and a large number of others have used inversion with compression of the floating ribs in artificial respiration, which has forced the blood into the chest and saved life again and again. For years the literature of medicine has teemed with reports of death from chloroform while the patient was sitting up or half recumbent, because the blood paths being dilated this posture favored anæmia of the vital centers. Again, it has been proved both experimentally and clinically that the best vaso-motor stimulant—belladonna or atropine—given before the chloroform, increases its safety, and that compression of the limbs by bandages does likewise. Finally, Hill has shown that abdominal compression also produces resuscitation by sending the blood to the heart. On the contrary, saline transfusion, which at first glance would seem to be indicated, is useless, because the dilated blood-paths will receive all the saline for a long time before they will overflow towards the heart, for as fast as the fluid flows in they dilate.

My conclusions therefore are, that while chloroform in its general depressing power depresses all vital functions, it is the question of blood-pressure which is most important, and therefore in the use of chloroform we should always keep the head low, precede the use of chloroform by atropine hypodermically, bandage the limbs if the case is feeble or already bloodless, and if necessary place compresses on the belly and apply them deeply by pressure if a failing circulation is developed.

The primary action of the chloroform is to depress the blood pressure chiefly by its vaso-motor effect, secondly by its cardiac effect, and finally that while the drug does exercise a depressant effect on the respiratory center, the failure of this center is chiefly due to anæmia. As, however, an intact respiratory center means regular breathing, we watch this function to determine the dose of chloroform actually inhaled, and because any variation in this function, as shown in irregular breathing, means that the chloroform is disordering arterial tension. Death from chloroform, then, is usually a vaso-motor death, for an intact arterial system is as important to vital function as an intact cardiac apparatus.—Therapeutic Gazette.

A BRIEF OUTLINE OF THE THEORY OF THE MOVEMENT OF THE NEURON AS APPLIED TO NORMAL AND PATHOLOGIC MENTAL AND NERVOUS PROCESSES.*

By F. X. DERCUM, M.D.,† Philadelphia.

Subsequent to the reading of the interesting paper on The Neuron by Dr. A. O. J. Kelly,[2] a number of members of the Society requested me to give an explanation of the theory of the movements of the neuron as held by myself. As I have pointed out in a previous paper, the theory that the neuron is not an absolutely fixed morphologic integer has been advanced independently by several writers. When the thought first occurred to me that nerve cells have the property of movement, I examined the literature and found that it had already been announced independently by three writers, one in Germany and two in France. The first to advance such a view was Rabl-Rückard, who in 1890 suggested that nerve cells have ameboid movement. At the same time he pointed out the significance of this view in enabling us to explain the phenomena of hysteria. His ideas, however, attracted no attention, but in 1894 Lépine, in relating a case of hysteria of peculiar form, advanced practically the same theory. His idea was that the neurons were capable of movement to such an extent as to enable them to alter the degree of their relation to each other. Some six months afterward another French writer, Mathias Duval, advanced the same theory in a communication made to the Societé de Biologie. Lépine had been unaware of the view advanced by Rabl-Rückard, and Mathias Duval was equally ignorant of that advanced by Lépine. A week after Duval had announced his theory, Lépine before the same society repeated his former arguments in its support. I myself presented the theory of the movement of the neuron in a paper read before the College of Physicians of Philadelphia, in January, 1896; and in June of that year read an address on the same subject before the American Neurological Association. In the meantime, in the spring of 1896, the theory had been again advanced by two other French physicians, Azoulay and Pupin.

This theory has now begun to attract general attention and is gradually meeting with support. It has been opposed, however, by the distinguished Spanish histologist, Ramon y Cajal. I will not take time to consider at length the views of this investigator, as I have already done so in a previous paper. I will, however, pause sufficiently long enough to point out that his argument bears within itself its own refutation. He maintains that nerve cells do not move, but on the other hand that the neuroglia cells move. He points out the fact that the processes of the neuroglia cells have numerous short aborescent and plumed collaterals, and he states that in these cells two different phases can be observed; first, a stage of contraction, that is a stage in which the cell processes become shortened; and secondly, a stage in which the cell is relaxed, that is a stage in which the processes of the neuroglia cells are elongated. He maintains that the processes of the neuroglia cells represent an insulating and non-conducting material and that during the stage of relaxation these processes penetrate between the aborizations of the nerve cells and their protoplasmic processes, and thus make difficult or impossible the passage of nerve currents; on the other hand, in the stage of contraction the processes of the neuroglia cells are retracted and they no longer separate the processes of the nerve cells, and the latter are thus permitted to come into contact. Evidently Ramon y Cajal admits the very thing against which he contends, for if the nerve cell processes are at one time not in contact and at another are in contact, they must certainly move, and the question before us is self-admitted. It matters not whether the processes of the nerve cells move little or move much, but that they move at all is the question at issue, and this Ramon y Cajal admits, though he makes the movement a purely passive one. It is certainly a minor point whether the movement of the processes of the nerve cell is active or passive; although it is in the highest degree improbable, and is certainly not evident from the histologic facts at our disposal, that the neuroglia corpuscles play the roll of an insulating material.

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* Read before the Philadelphia County Medical Society, February 24, 1897.

† Clinical Professor of Nervous Diseases in Jefferson Medical College; Neurologist to the Philadelphia Hospital.

[2] Transactions Philadelphia County Medical Society, 1896, p. 215.

While the movements of nerve cells have not been observed in vertebrate animals, such an observation has been made in an invertebrate form. A German investigator, Wiedersheim, in 1890 saw, in the living animal, leptodora hyalina, an entomostracon, the nerve cells in the esophageal ganglion move. This ganglion may in a sense be regarded as the brain of the animal, inasmuch as it receives the fibers of the optic nerve, and Wiedersheim actually saw these cells move and change their shape, and this movement he describes as slow and flowing. (He presents a number of drawings of the esophageal ganglion made at different times, showing the various shapes assumed by the nerve cells.) This observation evidently possesses a profound significance. It does not matter that it was made upon a form far removed from the vertebrates, for we should remember that it is just in the lower forms that general biologic truths have been demonstrated. I will not occupy time with a further consideration of the facts on which this theory rests, but will proceed at once to its application to normal and pathologic mental and nervous processes.

Before beginning a systematic consideration of the theory, it may be well to give as an illustration of its simplest expression the supposed behavior of the neurons in a case of transient hysterical paralysis. Hysterical paralysis, as we all know, may be so complete as to simulate very closely paralysis due to a gross lesion, and yet in the course of a few hours or perhaps in a few minutes such paralysis may absolutely disappear. Modern students of hysteria are convinced that hysteria is a bona fide affection with a syndrome that is as characteristic as that of any of the diseases with which we are acquainted. The idea of voluntary simulation has no hold on the minds of neurologists to-day. What is it that happens in the nervous system when a limb, say an arm, is suddenly paralyzed, remains paralyzed for a time and again suddenly recovers its functions? Evidently some change has taken place in the nervous system. Let us see how the theory of the mobility of the neuron enables us to explain the phenomena.

The neurons of the motor area of the cortex, as you are aware, present not only protoplasmic extensions directed towards the surface of the cortex, the dendrites, but also a protoplasmic extension downward, which becomes a nerve fiber, but which is, properly speaking, a process of the cell body, no matter what its length may be. This process, which is now known as an axon, extends downward through the white matter of the brain, the internal capsule, the crus, the pons, the medulla and the cord and terminates in the last in a brush-like extremity, the end-tuft. By means of this end-tuft the cortical cell is brought into relation with the motor nerve cells in the spinal cord. What the relation between this end-tuft and the nerve cell in the anterior horn of the spinal cord may be is not known. It is probably one of contact or of close proximity. That an actual continuity of structure between the end-tuft and the spinal cell does not obtain is now absolutely known.

Now what happens in the case of hysteria in which the arm has been paralyzed? As the result, we will say, of an emotional or other psychic shock, or possibly of a physical shock, the neurons of the arm center of the cortex retract their processes and in such a way that their end-tufts in the spinal cord no longer bear their normal relation to the spinal neurons. In other words, the connection between the cells is broken. We will suppose now, as many of us have seen, that as a result of suggestion, either with or without hypnotism, or spontaneously, the paralysis of the arm disappears. According to the view here advanced this disappearance is due to the extension or protraction of the processes previously retracted—that is, the end-tufts resume their normal relations with the spinal neurons and function is re-established. Certainly no other theory explains so readily the appearance and disappearance of hysterical paralysis or of hysterical anæsthesia. In these conditions there is really a lesion, consisting of a break in the normal relations of the neurons, due to retraction of cell processes.

As regards the movement of the cell processes, it is highly probable that this property is not limited to the end-tufts of the axons, but that it is also shared by other processes of the nerve cell, especially the collaterals, and perhaps even, to some extent, by the dendrites. A further possibility suggests itself, and that is, that the movement may be not simply one of retraction or protraction, but also lateral in direction.

Having acquired a clear conception of what is meant by the movement of nerve cells, let us proceed to apply the theory in a systematic manner and let us see how readily it enables us to explain various normal and pathologic phenomena. Let us take, to begin with, the familiar instance of sleep. Sleep has been commonly attributed by physiologists to an anæmia of the brain, notwithstanding the fact that true and undoubted anæmia of the brain, such as follows great loss of blood, or accompanies marked general anæmia is not attended with somnolence, but rather with insomnia. Let us see how the movements of the neurons will account for sleep. Evidently the neurons, when functionally active, must be in relation with one another. Their processes must either be in contact or nearly so. Evidently this condition is a perquisite of consciousness. Now what happens when the nerve cells are exhausted by fatigue, when their volume and their cell contents have been diminished, as we have every reason to infer is the case, from the experiments of Hodge? Evidently their processes become retracted and they are no longer in relation with one another. The neuron isolated from the rest by retraction must be without function, must mean absolute loss of function, must mean unconsciousness, must mean sleep. In other words, in sleep the neurons have their processes retracted; in consciousness their processes are extended.

See, also, how beautifully this theory enables us to explain pathologic unconsciousness, such, for instance, as attends cerebral concussion. A man, while conscious, receives a severe blow upon the head; the neurons suddenly retract their processes, and unconsciousness results. Certainly no other theory for the explanation of the unconciousness of cerebral concussion is equally simple, or equally plausible. It is extremely probable also that other forms of unconsciousness are to be similarly explained; for instance, the unconsciousness of chloroform or ether anæsthesia, or the sleep produced by drugs. In these conditions it would appear that the neurons, by virtue of the direct action of the substances introduced into the circulation, retract their processes, and, for the time being, sever their relations with one another.

Let us now turn our attention briefly to elementary normal psychic processes; let us take up, for instance, the physiology of perception. What are the changes that take place in the neurons when a thing is felt or seen? Let us take a simple illustration, such as I have used in a previous paper. Let us assume that a sequence of sound vibrations impinging upon the peripheral auditory neurons—that is, the nerve cells of the spiral ganglion of the cochlea—produces in these neurons a change that affects the relations which their neuraxons—that is, their centripetal processes—bear to the nerve cells of the auditory nuclei of the medulla, and secondarily to the neurons in the auditory area of the cortex. It is probable that the effect produced upon the latter is such as to cause them to retract, extend or otherwise move their processes, and thus bring about a change in the relations which the various neurons of the auditory area of the cortex bear to one another. It is evident, further, that the same sequence of sound vibrations must always produce the same changes in the cortical neurons. In other words, given sounds are represented by certain combinations of the neurons with one another, and these combinations persist only so long as the vibrations impinge upon the auditory apparatus. An act of perception, therefore, consists of changes in the relations of the cortical neurons with one another; these changes corresponding to, and depending upon, certain physical impacts upon the sensory organs. As a corollary, the combinations among the neurons so formed bear definite and fixed relations to the external world.

Now what is the physiology of a conception? Evidently it consists in the reforming among the neurons of an old combination—that is, a combination the same as, or similar to, what was originally produced by a physical impression upon the organs of sense, but which is now produced by some other agency. Thus, a given note produces a certain combination among the neurons in the auditory area, and the forming of this combination is an essential part of the act of perception. To my mind, the act of conception consists in the formation among the auditory neurons of the same combination, without the actual impinging of the sound vibrations upon the ear. The explanation of the reproduction of old combinations in the cortex, without external sensory stimuli, I will, for a moment, defer, and pass to the consideration of memory.

Various theories have been advanced to explain the phenomena of memory, but I will not take time to consider them, simply showing how readily the theory of the mobility of the neuron explains this remarkable property of the mind. Let us recur for a moment to the illustration just used. We will say that a sequence of sound vibrations has impinged upon the peripheral auditory neurons, and, in turn, has caused the neurons of the auditory cortex to move their processes. Now, other things equal, the degree of the excitation depends upon the intensity of the physical impact upon the auditory apparatus. Evidently the number of auditory neurons aroused into action must vary greatly, and it is probable that the movement does not cease in the auditory area of the cortex, but is diffused among neighboring areas, and even among those which are relatively distant. Now, we will suppose that a sequence of sound vibrations has caused the auditory neurons to assume new relations with one another. These new relations will depend largely upon the fact whether or not a similar sequence of impressions has passed through those neurons before. If so, the old combinations will be re-formed and, as a corollary, there follows the recognition by the ego of the sounds as something heard before. Here we have an elementary illustration of the act of memory. According to this view, memory does not depend upon fixed cerebral structure, but upon the combinations of the neurons formed from time to time being the same as, or similar to, those that have been formed before. Let me illustrate a little further: the excitation that has been produced in the auditory area of the cortex gradually diffuses in various directions—that is, there passes from the auditory center of the cortex, through the general cortex of the brain, a series of combinations among the neurons along the oldest and best traveled lines. At first a given sequence of musical sounds suggests merely a familiar air, but, a moment later, when the movement has diffused as far as the visual areas of the cortex, old combinations are re-formed, which now give rise to visual memories, and instead of merely the memory of a familiar air, we have in addition a visual memory of an instrument, or a person with whom the air is associated, or, perhaps, of an operatic performance. I might enlarge upon this theory of memory, but time will hardly permit. Let us bear clearly in mind, however, that memory is not due to any constant condition of structure, but merely to the forming anew of old combinations among the neurons.

Other psychic phenomena also find a ready explanation on the theory here advanced. We can readily understand, for instance, how sequence of thought is explained by the movements of the neurons. If the neurons are constantly changing, if from the hour of awakening they are constantly responding to stimuli from without by changes in their relation to one another, it follows that the sequence of thought itself depends upon the sequence of these changes. Herein lies, also, the explanation of the continuity of thought. The direction of thought, and the association of ideas, must depend largely upon the frequency, and the consequent readiness with which special combinations among the neurons have been and are formed. Another idea also suggested in this connection is that not all of the neurons are active at the same time, but that the number varies greatly. However, I shall not pause to further consider this aspect of the subject.

I shall now call your attention briefly to the application of this theory to various pathologic conditions. Let us take up some of the simplest phenomena observed in insanity. What is an hallucination? An hallucination I would define as the spontaneous formation of an old combination amongst the neurons representing a former perception or conception, as the result of pathologic conditions, and not in response to normal stimuli either from without, such as physical impacts upon the sensory organs, or in response to normal stimuli from within, such as preceding and connected trains of thought, or other normal psychic processes. The combination representing an hallucination reacts in such a manner upon the general neurons of the cortex as to give rise to the belief in the reality of the sound, vision or other special sense manifestations.

What is an illusion? An illusion is such a faulty combination of the neurons among each other as to lead to an imperfect recognition by the ego of the object perceived. Take, for instance, a case in which a cord or rope is mistaken for a snake. Here we have physical impacts normally received by the sensatory apparatus, but owing to a diseased condition of the neurons, these impacts lead to incomplete and erroneous combinations. We have already learned from the investigations of Berkeley, Andriezen and others that the neurons, as a result of the action of various toxins, such, for instance, as alcohol, may become diseased, and especially their processes. There would, therefore, seem to be every justification for regarding illusions as dependent upon faulty combinations among the neurons.

What is the pathology of a delusion? Here again it is probable that the explanation is to be sought in disease of the structure of the neuron, and especially of its processes. It is characteristic of a delusion that the patient is incapable of accepting evidence in relation to his false belief, even when that evidence is properly presented. It would seem that in the case of a delusion it is impossible for the neurons to form such combinations with each other as will bring the patient into proper relations with the outer world. A simple illustration will make my meaning clear. The man who believes, for instance, that he has no mouth, in spite of the most direct physical evidence to the contrary, presents an instance in which a lesion has taken place among the neurons which renders them incapable of forming normal combinations in response to physical impacts upon the sensory organs. Again, it may be that a correct combination representing a perception or a conception is formed, but in such case the neurons of adjoining areas are defective, in that they cannot bring this combination into normal relation with the general neurons of the cortex.

How can we explain such a symptom as delirium, or the mental condition seen in confusional insanity? Evidently owing to disease of the neurons, the disease being transient and due to the presence of toxic products in the case of delirium, or being possibly structural in the case of confusional insanity, the neurons act irregularly and wildly, making constantly the most erroneous combinations with one another. Fully formed and partially or erroneously formed combinations rapidly succeed one another; probably also without relation to previously established lines of diffusion. It is easy also to explain the expansive mental state of mania and the depressive state of melancholia by merely supposing in mania that as a result of pathologic causes, generally toxic disturbances of nutrition, the neurons are acting with excessive rapidity and that a greater number of them are active than under normal conditions; and in melancholia that the movements are restricted and slower, and that a smaller number of neurons are active.

I had intended also to show the application of this theory to such pathologic conditions of the nervous system as are seen in hysteria and hypnotism. I have already instanced its application to one phase of hysteria, namely, hysterical paralysis. Let it suffice to say that all of the symptoms presented by hysteria, even the convulsions, can be explained by it. I will not, however, more than allude to this portion of the subject. To hypnotism, however, I will devote a few words, inasmuch as the theory of the mobility of the neuron is the only one that affords a rational explanation of this phenomenon. Let us recall to our minds for a moment the condition of the neurons during sleep: their processes are retracted. Hypnotism is partial sleep—that is, a state in which only a portion of the neurons have their processes retracted. Let me illustrate my meaning a little more in detail. Let us take a case in which hypnosis is induced by the ordinary method of staring fixedly at an object and in which at the same time the suggestion of sleep is made verbally by the operator. Now, by reason of the effort of the attention, expended through both the visual and auditory channels, the neurons of the corresponding regions in the cortex are thrown into certain relations with one another, relations that correspond first to the action upon the ocular apparatus, and secondly, to the words of the operator received through the auditory channels. Very soon, by reason of the visual fatigue and under the auditory suggestion of sleep, the neurons of the visual area retract their processes and the partial sleep of hypnosis begins. Gradually the sleep grows deeper—that is, by reason of the same influence, the other neurons of the cortex gradually retract their processes, with the single exception of those by means of which the verbal suggestion of sleep has been received, namely, the auditory channels. In other words, the consciousness of the individual has been restricted to the narrow field of auditory perceptions. While in this condition, somnambulism is readily induced. In response to words from the experimenter certain movements are induced among the neurons in the auditory cortex, and these movements, being communicated gradually to the neurons of other areas of the cortex, for instance the motor area, may lead the individual to perform certain automatic acts or manifest signs of cerebral activity referable to other portions of the cortex.

I might still further elaborate this most interesting subject, but I must content myself by merely indicating the method by means of which hypnotism is to be explained. To my mind the theory here advanced is the only one that satisfies the facts.

I have endeavored to present in this paper my own views as to the application of the theory of the movement of the neuron and in as condensed manner as possible. Any one of the special applications I have indicated could be enormously expanded. My desire, however, was to present this evening an outline merely of the subject, and I think that all will agree with me that the theory of the movement of the neuron is not only plausible, but captivating in the extreme, as it makes clear and comprehensible much that otherwise cannot be understood at all.—The Medical & Surgical Reporter.

THE DISGUISES OF TYPHOID FEVER.

By WILLIAM OSLER, M.D.,

Professor of the Principles and Practice of Medicine, Johns Hopkins University.

Report of Remarks Made at the Conference of Health Officers, held at Baltimore, February 17 and 18, 1897.

The older we grow the less we know. That at any rate is what I feel at times when I come up against the blank wall of a case of fever; is it typhoid, malaria or remittent? Now it is often a very difficult matter to make a satisfactory diagnosis early in typhoid fever and the diagnosis is sometimes not made. We have had, I think, out of about 530 cases, with I forget how many deaths, two cases in which the diagnosis was made by Dr. Welch in the pathological laboratory and not made in the wards. There are a great many cases during the year in the country in which the diagnosis is never made correctly *intra vatim*, but only post-mortem. The doctor is led astray in the diagnosis of typhoid in three main directions; first, its localization may deceive him. Typhoid fever may have a text-book symptomatology, or it may have no single suggestion of anything given in the text-book. It may, in the first place, begin with such marked cerebro-spinal manifestations that the physician is thrown off at once. It may not enter into his mind during a ten or twelve days' attendance upon a case that he is dealing with anything other than cerebro-spinal meningitis and a certain number of the cases of cerebro-spinal meningitis reported by Dr. McShane, I will not say how many, but I would like to say a great many, are typhoid fever, with cerebro-spinal localization.

Beginning with headache, marked retraction of the head and neck, and in fact not a single feature of cerebro-spinal meningitis lacking, it is so clear to you that you do not care to look for the spots or examine the spleen. I am not thinking of theoretical cases, but cases which I have followed myself not only to the clinic, but to the autopsy room. I have records of some three or four cases in which a diagnosis of cerebro-spinal meningitis is made, but the autopsy clinched the diagnosis of typhoid. We have had one case here in which not only were the symptoms suggestive, but in addition to the spasm of the muscles of the neck and back he had clonic contractions of the head and arm.

Another localization is in the lungs or pleura. You see the case as one or pneumonia. The patient's first indication is a stitch in the side; you have a friction murmur and you are thrown off your guard entirely. The pleurisy cases are not so common as the pneumonia. We have had one well marked case in which the symptoms were characteristic of pleurisy and it was admitted and treated so for one week. The pneumonia cases begin with a pain in the side, rusty expectoration, high temperature, tubular breathing and indications of the involvement of a lobe or the entire lung, and are more deceptive. You give a diagnosis of pneumonia; so it is, but about the tenth day, when you expect the crisis, it does not come; the temperature develops, the abdomen becomes tender and you have the extreme pleasure of telling the family that typhoid symptoms are developing. Now it is a blessing that Louis invented the name "typhoid symptoms." One of our errors of diagnosis was a case of that kind; an old man admitted with all the symptoms of pneumonia. It did not enter our minds for a moment that he had anything else than pneumonia until Dr. Welch examined him and then it was evident that he had typhoid fever.

Another mode of disguise is acute nephritis. That is fortunately rare, for the cases are almost invariably diagnosed as acute nephritis and not considered typhoid until a week or ten days have elapsed. Those are the chief errors of diagnosis, and it is a point that I often find practitioners have not had their attention sufficiently called to: namely, that typhoid may begin this way.

There are certain conditions that simulate typhoid fever. The simple continued fever, about which we have heard so much, is a fever that runs a week or ten days. You are safe to follow Murchison's opinion that the fever with local manifestations persisting ten days in a temperate latitude is likely to be typhoid. You must bear in mind that there are cases of typhoid with fever as the single manifestation, no swelling of the abdomen, no headache, patient feeling well and anxious to get up and you may have the diagnosis demonstrated in a very unpleasant way; the patient being allowed to get up, perforation may occur and hæmorrhage follow, as I saw in a case recently. We must have had six or seven cases in the past seven years in which the diagnosis of the fever was not made in the preliminary fever for which the patient came into the hospital. The temperature dropped two or three days after coming into the hospital, a period of apyrexia of three or four days and then the original disease was determined by the onset of a relapse.

There is only one disease in this latitude with which typhoid is likely to be confounded and that is the estivo-autumnal intermittent fever. Now when I tell you that only a few months ago we treated a case for five or six days, the blood examination had been made, not by Dr. Thayer or myself, I might remark, but it was made and it was not until the sixth day that we could determine that we had been tubbing and treating a case of malaria; you will understand that the similarity is very striking. In the first place take the mode of onset; as a rule in typhoid fever there is a much longer period of malaise. The patient is out of sorts for a long period and epistaxis occurs frequently. Herpes is a complication of typhoid and it is one of the commonest in malaria.

Chills and a chilly sensation are very common in both typhoid and malaria, the number of cases of typhoid beginning with chills being very considerable. Positive chills occurring at intervals of two or three days are more common, of course, in malaria, but if you take in consideration the chilly feelings they occur in about 50 per cent. of the cases of typhoid. There is a very great difference if you see the cases early, as you do generally in private practice, in the first or second week of typhoid or malaria. In malaria at the onset there are positive intermissions in the first week; in typhoid there are none; it is a gradual slope upwards. In the second week the difference is very marked. There is no single disease in which so steady a fever is present as in typhoid. We have now dozens of charts, even from cases under treatment by cold baths, in which the diurnal range did not exceed one and a half or two degrees.

Examination of the blood is, of course, an important point, particularly now since the Vidal serum diagnosis method. In a certain number of cases of malaria, remittent and continued fevers, the examination of the blood has to be made repeatedly and often at short intervals, for the parasites are not present in the circulating blood in large numbers. It is very important that those who are to make examinations of the blood in malaria should have proper preliminary training.

Is there a typho-malarial fever? Yes, in the brains of the doctors, but not in the bodies of the patients. There is no combined hybrid disease and it is only due to Woodward to say that he did not recognize a hybrid disease. Typho-malaria is a villainous name and should be banished from our vocabularies, and no doctor should ever use it, particularly to his patients. It gives a man a wrong sense of security and the doctor wastes a lot of good medicine, a lot of quinine, for instance, because he thinks there is some symptom that points to malaria. I am happy to say that cases of typho-malaria are disappearing slowly from the health reports; they ought to be banished entirely. Chills, as I told you, occur frequently at the outset of a disease and they may occur throughout the course of a typhoid fever. The State boards of health hereafter should return to every physician who sends in a diagnosis of typho-malaria his blank and ask for something better. It is too late in the day, gentlemen, to make that diagnosis.—Maryland Medical Journal.

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ABSTRACTS.

THE TREATMENT OF PERSISTENT VOMITING IN PREGNANCY.

According to Dr. M. A. Schab (La Presse Medicale, December 6, 1896), the enormous number of therapeutic means reputed to be valuable in the treatment of vomiting of pregnancy suggests their general impotency, as the more difficult a complaint is to treat the more numerous will be the remedies proposed.

The prevailing opinion seems to be that incorrigible nausea of pregnancy is a reflex neurosis of the stomach. The reflex originates in the uterus and terminates in its action on the stomach, which must always be supposed to be in a favorable neuropathic condition. A favorable nervous organism must exist for the evolution of the symptoms. A large number of individuals who vomit persistently during pregnancy have hysterical tendencies. The symptoms begin and cease suddenly without apparent cause.

The treatments proposed have been hygienic, medical, dietetic and local.

Hygienic and Dietetic Treatment.—The treatment often produces excellent results. In view of the fact most of the patients are neuropathic and hysterical, hygienic treatment becomes most important and rational. Frequently the most simple practice will arrest the vomiting. A change of habitation with a sojourn in the country is always to be recommended.

Very unimportant changes sometimes cause the persistent vomiting to cease. The departure of some person from the family circle, the experience of a strong moral emotion, or an intercurrence of a sickness will cut short the vomiting. Also a mental influence exerted by a stranger or by the physician will be very beneficial.

Alimentation is not less in importance than hygienic and moral influences. One point must always be insisted upon, namely, that the diet be composed of a great variety of foods. With this variety those which are best borne by the stomach can be discovered.

Beginning with the foods most easily digested the capricious appetite can be led to tolerate meats, etc. It is well to begin with a simple diet. Milk and liquid foods can most always be borne. Sometimes the milk will be agreeable if it is flavored with peppermint, etc. Often the best results can be obtained with alcoholic drinks. The effect of the alcohol on the circulation, its stimulation of the gastric secretion and its promotion of the muscular movements make it a valuable agent. Some form of wine, in small doses, after meals, is therefore good. Cazeaux recommends large doses.

Medical Treatment.—There is almost no end to the list of drugs proposed for persistent vomiting of pregnancy.

It is well to attempt to combat the vomiting of pregnancy by the means employed in cases of nausea from ordinary causes.

All drinks which liberate carbon dioxide are worthy of mention. The gas acts with great efficiency. It is on account of this gas that champagne is so well tolerated.

The alkalies (sodium and potassium salts) cause an increased action of the gastric glands and they, therefore, have a good effect on nutrition. Sodium bicarbonate in fifteen-grain doses is the best salt. The alkaline waters are also good.

Lusk advocates the use of nux vomica, which stimulates the muscles of the stomach. Five to ten drops after meals is the proper dose. Tincture of iodine has been extensively used by Lasegue. Pepsin has its advocates.

Germain See has proposed the use of cannabis indica in moderate doses. Very often sulphuric ether, taken on sugar or in pearls, moderates the persistent vomiting.

Tarnier considers lavage of the stomach to be the most rational means of treatment.

It is a general observation that constipation is a troublesome complication in cases of persistent gravid vomiting. Laxatives and purgatives are therefore called for.

There is a list of drugs which have a special action on the muscles and mucous membrane of the stomach. The action on the mucous surface is anæsthetic. These agents often act very efficiently against the vomiting. These drugs are chloroform, cocaine, opiates and menthol. The chloroform can be administered in an aqueous solution flavored with mint or orange water.

Cocaine has been extensively used with success.

Opiates have been strongly recommended by German authors. Enemas of tr. opii or hypodermic injections of morphine, give better results than does an oral administration of the drug.

There is another line of treatment based on the assumption that the pathology of the vomiting is to be found in a hysterical state. From these premises it is natural that a treatment should be proposed which acts as a calmant to the general nervous system. Bromides and chloral introduced by the mouth or rectum often yield excellent results. According to Cohnstein, the bromides should not be continued if the vomiting does not cease after a few doses. The best results are obtained in the early months of pregnancy. When the vomiting habit is in its inception chloral must be given in large doses. It acts on the vomiting by causing muscular depression.

The use of cerium salts has been highly extolled by Simpson. The salts used are the oxalate, carbonate or valerianate. It is not known how these salts act, but they are positively beneficial in many instances.

Many have recourse to drugs which stimulate the general nervous system. Caffeine, injections of ether, and artificial serum have been used with seeming success. The serum has demonstrated its worth in cases where vomiting is associated with malnutrition.

Very often adjuvant external treatment has been practised. This consists, namely, in revulsive applications on the epigastric region and along the spine.

Treatment with electricity has many advocates. Faradization has been used with varying results. Some observers are inclined to credit the continuous current with all the good results. As early as 1868 Semnola practiced systematic electrical treatment in all cases of nervous vomiting, and he has reported cases of vomiting of pregnancy which yielded to the application of the current to the epigastric region.

For a long time it has been held that the reflex cause of the vomiting lies in the uterus. Consequently local treatment has been important.

It was thought that a hyperæsthesia of the neck from a congestion was the cause, and the neck has been sacrificed with hopes of betterment.

Cervical ulcerations have borne the blame for the trouble and they have been cauterized with silver nitrates, etc.

Some authors have favorably modified the vomiting by correcting malpositions of the uterus.

Digital dilatation has been practiced by Copeman.—Medical Review.

There are reported from March 1, 1894, to March 1, 1895, from Sweden, Norway, Denmark and Finland, 15,052 anæsthesias. Of these, 11,284 were with chloroform, 1,279 were with ether, 2,122 with mixtures of chloroform and ether, 362 with ethyl bromide, and five with chloroform and ethyl bromide.

The administration of chloroform has been by drops, by large doses, with preparatory injections of morphine, without morphine, after the administration of cocaine by the mouth, and with preparatory introduction of cocaine by the nares. The difference observed between the administration of chloroform by drops and by large quantity was insignificant.

Morbid symptoms were observed during and after narcosis as noted.

Vomiting during the anæsthesia occurred as follows:

	Per cent.
Ether, without injection of morphine	14
Ether, with injection of morphine	25
Ether and chloroform without morphine	13.5
Ether and chloroform with morphine	18.1
Chloroform by drops without morphine	10.5
Chloroform in large doses with morphine	14.8
Chloroform, with cocaine in the nares	13.6
Bromo ethyl	10.77

Asphyxia during narcosis was as follows:

	Per cent.
Chloroform, with cocaine by nares	3
Chloroform in large doses with morphine	1.7
Chloroform in large doses without morphine	1.3
Chloroform by drops without morphine	1.03
Chloroform and ether with morphine	18.1
Chloroform and ether without morphine	0.96
Ether without morphine	0.79

Syncope has been observed as follows:

	Per cent.
Ether without morphine	0.08
Ether and chloroform without morphine	0.58
Chloroform by drops without morphine	0.4
Chloroform in large doses without morphine	0.2

Asphyxia has been observed in 139 cases, syncope in 52 cases. There were 5 deaths during chloroform narcosis—one in 2,257 cases.

Vomiting after narcosis was observed in the following proportions:

	Per cent.
Ether with morphine	25
Ether without morphine	24
Ether with morphine and atropine	8
Ether and chloroform without morphine	26.6
Ether and chloroform by drops, without morphine	20
Chloroform by drops with morphine	6.15
Chloroform by drops with injection of cocaine in nares	42
Chloroform in large doses without morphine	57
Chloroform in large doses with morphine	40
Chloroform, with cocaine by mouth	2.6
Bromo ethyl	1.1

Jaundice followed chloroform narcosis in 10 cases.

Somnolence was long continued in 58 cases.

Albuminuria has been observed 128 times, but was not confined to either agent.

There were 36 cases of pulmonary affections after narcosis.

There were 43 cases of feebleness of the heart and collapse after narcosis, as follows:

	Per cent.
Chloroform in large doses without morphine	.39
Chloroform by drops without morphine	.32
Chloroform and ether without morphine	.51
Ether without morphine	.15

Eighteen deaths following and to a certain extent attributable to the narcosis occurred as follows:

	Per cent.
Chloroform by drops with morphine, 2 cases	2
Chloroform by drops without morphine, 8 cases	.08
Chloroform in large doses without morphine, 2 cases	.18
Chloroform and ether without morphine, 4 cases	.25
Ether without morphine, 2 cases	.15

Hospital Physician Enydahl states that he has not had time to make a tabulated report. He is in favor of ether, and says that the reason why it sometimes does not work well is that the ether of the shops is not pure. It must be absolutely pure. He uses Shiring's. He has administered ether (always without morphine) in at least 2,000 cases, and has never had a death. He gives it always fasting, or after a cup of tea. Always drops it on a sponge in a napkin and brings it gradually and not smotheringly over the face. He has never had any serious complications of the respiratory organs. Sometimes, if the patient coughs badly, he changes to chloroform, after which the patient will take ether well, but he never prefaces ether with chloroform, as some patients will die at the very commencement of chloroform inhalation. He attributes his great success to the absolute purity of his ether.

He says that if, in using chloroform, the patient breathes very heavily, a change to ether will make the breathing lighter.—Medical and Surgical Reporter, April 18, 1896.

THE ORGANISM OF SYPHILIS.

Van Niessen (reprint from Wien. Med. Woch., Nos. 36-40, 1896), replies at some length to the critics of his book on the syphilis bacillus and adduces further evidence in support of his views. His later work has been carried on mainly in two directions: First, the comparison of micro-organism stained in syphilitic tissues with those observed in pure cultures; and, secondly, the cultivation of the specific bacillus from the affected tissues. He has especially investigated syphilitic lesions of the brain and spinal cord. Sections of these were stained by Gram's method or cultivations made by imbedding little pieces in gelatine, so that one surface was left exposed to the light. On this numerous colonies soon developed, which were employed in further researches. The author claims to have infected eight rabbits in various ways, so as to produce secondary syphilitic lesions, but he has not yet succeeded in inducing the primary lesion upon the genitals. Owing to the great difficulty of excluding other causes, such as infection by tubercle and coccidia, his researches in these directions are not yet ripe for publication. He has, however, been able to demonstrate the "syphilococcus" in an exercised primary sore of the prepuce, and also to cultivate it in gelatine mixed with the blood taken from the wound produced by the excision. The cocci were present throughout the tissues removed, but were particularly abundant, often forming emboli, in the deeper layers. Van Niessen is therefore convinced of the diagnostic value of the organism in primary affections, and although the result with later syphilitic lesions of the central nervous system are not as yet equally consistent, he is convinced that further improvements in investigation will render them equally available for diagnosis. His inoculation experiments are very striking. A pure culture of the organism, from the primary sore above mentioned, on veal broth was injected two months later into three rabbits, a goat, a guinea-pig, and a pigeon. In several of the animals typical hard sores developed, which in the pigeon took on a phagedenic course, while in the goat and one of the rabbits gummata developed. The syphilis thus produced in animals was characterized by slow and protracted symptoms, by a marked tendency to cell proliferation in the form of a tumor with necrotic centers, and by involvement of the capillaries and lymphatics. The author claims to have proved that his syphilococcus when injected in pure culture into animals will produce both the typical primary affection and gummatous tumors.—British Medical Journal.

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PROCEEDINGS OF SOCIETIES.

CINCINNATI OBSTETRICAL SOCIETY, DECEMBER 10, 1896.

REPORT OF CASES.

Dr. Reamy:—Mr. President: Of course it is very well known to you all, who have paid any attention to the subject, that it is nearly fifty years since a physician of Paris first removed an interstitial fibroma from the cervix by morselmon. From that time on, or a long time, French literature was pretty well filled with reports of operations of this character. In 1853, in that celebrated prize essay to the American Medical Association, Dr. Atlee, of Philadelphia, discussed this subject and reported cases. There is scarcely any generally accepted text-book on operative gynæcology in which there is not at least a part of a page devoted to the discussion of this method of removing these tumors. I suppose there is not one present who has not removed tumors in this way. I make this mention of the matter for the reason that in the last decade the brilliancy of abdominal surgery, the greater facility and ease and comparative safety attending abdominal hysterectomy, has led operators to a very large extent not only to discontinue this method of treating interstitial tumors, small and large, especially the large ones, but judging from the fact that some time ago, when I reported a case similar to the one I shall call attention to now, some members of this Society discussed it as though the method were new and novel, I assume it is about to become a lost art. Twenty years ago I was doing a large number of operations by the various methods, by divulsion and morselmon, and also by enucleation, and I myself allowed the operation to come to a large extent into disuse in my own work until about nine years ago. In the last two years I have revived it with a good deal of vigor in my practice. I have recently had some very interesting cases. The specimen which I present here represents a case of as much interest as any I have had. I would like to say a few words as to the essential elements entering into success in such cases. These masses of fibroid tissue before you represent what was saved—shrunken, of course, about one-half by alcohol—from a tumor quite as large as a fœtal head and which with the uterus filled the entire pelvis, and was not only interstitial, but so filled the posterior and upper portion of the uterus that the uterine cavity was only about three inches in depth, the anterior lip being very much attenuated and thin and the posterior lip not recognizable. The tumor filled the posterior part, so that posteriorly it was impossible to distinguish by visual examination or by touch the commencement of the uterine tissue proper. The woman was 49 years of age, a resident of a city in Missouri, a woman in the upper walks of life and the mother of several children. I commenced the preparation of the patient by attempting to dilate, by carrying my finger back and forth and trying to separate the uterine tissue. I found I made little progress in this way. I also attempted to dilate with a uterine steel dilator, but the walls were so thin there was danger of lacerating them. When I had carried this manipulation as far as I dared and saw I would not be able to accomplish anything more, I put the patient under an anæsthetic and made a slight operation, which I have been accustomed to do in these cases and which I would be glad to call attention to as probably original with myself. With the patient under an anæsthetic I brought the uterus down as well as I could, for the os was under the pubic arch, and by carrying the finger into the rectum and pushing the fundus forward as much as possible (it was almost immobile), I took the site which I thought was about the proper position for the posterior lip, and cut through with a bistoury and made a posterior lip, cutting the capsule at the lower segment of the tumor. I seized the tumor then with a vulsellum and dragged upon it until I got the uterus down. Then with an ordinary pair of curved scissors I went beyond that a little and removed some tissue. This small specimen represents all I removed at that operation. I hollowed this out in the center of the tumor just beyond the incision I made. This incision, of course, cut off the vascular supply largely from the lower portion. You are all aware that these tumors are supplied largely upon the periphery. A little temperature was found following the operation. I gave the patient no ergot. I continued my manipulations and in a short time could find the posterior lip. I then waited several weeks, and under that stimulation and the surgical procedure of cutting off the blood supply and separating the tumor for the distance of probably an inch, the uterine action, for she several times had severe pain, separated it still further. Afterward I was able to do the final operation, which consisted in the removal of the tumor by morselmon. Every operator, every author whom I know of who recommends this operation, regards it a very important matter if you can bring the masses out as an entirety. In the case I reported to this Society at Dr. Mitchell's, I bisected the tumor, then quartered it and brought out one quarter at a time. In a tumor as large as this one, and especially when you are pretty certain the walls of the uterus are thin, you cannot remove it in that way even if you were to incise the cervix bilaterally even to the os internum. It is better to remove them by piecemeal.

I find by attempting this method, that following as far up as I had succeeded in cutting the cervix and tumor detached by an operation I have before mentioned and by the changes that occurred subsequently as the result of that procedure—and it is to that I wish to call especial attention—I then seized it and took away from the uterus all I had separated. Instead of retaining the smaller end of the cone, I amputated it, so as to give me a larger command of the tumor. I then hollowed it out. Of course I had dilated the cervix as much as possible under an anæsthetic. I hollowed out the tumor and did not seek to enucleate it. Then, instead of having the large mass, I had the tumor hollowed almost to the fundus. I then seized it with the Pean forceps, which are better for that than for any other purpose for which I have used them. I then slipped my finger between the tumor and the uterus and enucleated a little path and cut that away, and then I went on around. When you have this operation about one-half done, you have the uterus inverted about one-half. When the uterine walls are so attenuated as they were in this case, perhaps in some places only one-tenth of an inch thick, you can make the separation under the eye. There is in every one of these cases at every part of the periphery of the tumor, always the well-known anatomical structure of a capsule. It is not true that in any of these cases the fibers of the tumor destroy entirely the uterine fibers so there is only the peritoneal covering left. It is immaterial if the uterus is so thin that when you have it away it falls as if there were only left the peritoneum. The safety of the operation consists in the recognition of that condition and in removing only little pieces at a time. This larger round mass was the last I got from the fundus of the uterus. The only objection is that it takes too long to dilate, but nothing can take too long when we are dealing with a case of this kind. If you get the center of the tumor well removed, the dilatation need not be so extensive as when you remove a tumor in halves or quarters. You can bring the uterus down and know when you have all the tumor removed and that you have not ruptured the uterus. When I had this tumor all removed by this method, I mopped the uterus out with sterilized gauze and put in two yards of iodoform gauze, carrying it in in two separate pieces, not rolling it up very tight, and filled the cavity pretty thoroughly.

Drs. Bonnifield and Gillespie were kindly present and assisted me. The patient will be able to go home before Christmas. An interesting feature is, that following each one of the preliminary operations the patient showed signs of sepsis. On the evening of the third day after the final operations, when I removed the gauze many parts of it were dry, not even stained.

Second Specimen: This little specimen is interesting, especially in one particular. The patient, a woman æt. 45, has never borne children. She is quite fleshy, but otherwise in good health. Her residence is in a town in the eastern part of this State, and when she first came to me there appeared to be a pronounced cachexia. She stated that she had an unpleasant discharge and was suffering from uterine trouble. I examined her, and when I found the discharge was offensive and the uterus enlarged, I inferred she had a carcinoma or sarcoma of the corpus uteri. I made up my mind that as a preparatory step, and not that I thought I was mistaken in my diagnosis, I would thoroughly curette the uterus. When I dilated and introduced the curette I found a body in the uterus. This mass, an intra-uterine fibroma, was then twice as large as it is now. There seemed to be a large amount of material in the pelvis, other than the uterus and its contents. When I cut this out I was very greatly gratified at the simplification of the difficulty, and the facility with which I had gotten rid of what I believed to be a malignant disease. The operation was done about four weeks ago, and the uterus is now reduced in size so that it is only about one-half as large as it was before. I report the case because I was by no means clear in my diagnosis, and if there is any comfort in such a thing I found it just the other day, within the last week, when I learned that this patient had been in the hospital of a friend of mine, a man of very large experience and great ability as a diagnostician and operator, who is present to-night but does not know anything about it. I have not had an opportunity to speak to my friend about the case, but I believe just before the time for operation the patient left his hospital, perhaps under the influence of some friends.

Third Specimen: I present this little specimen simply to call attention to symptoms that followed the section, not unique, but somewhat out of the ordinary. The reporting of every case of the kind with results, contributes to the general knowledge of the subject. The woman from which this was removed is a virgin and had an enlarged uterus with a very constant discharge and a good deal of menorrhagia and pain. A year ago I curetted and packed and the patient got better, but did not recover and the symptoms recurred. She came down here from Fayette county, this State, and I operated last Friday. She had intense pain and I could feel very distinctly on the left side a mass, which I took to be the ovary very low down. The uterus was enlarged and unusually hard. I thought at first of a vaginal hysterectomy, and then I thought I would open the abdomen and see what I could find. On opening the abdomen I found the ovaries moderately adherent to everything they could touch. The pelvis was pretty well blocked off. I removed the ovaries, not because they were diseased, but because of the condition of the uterus; otherwise, I would have broken up the adhesions and removed the cyst from one of the ovaries. But the uterus was so intensely hard I concluded it was not wise to leave them. The pedicles were small and I used a very small ligature and transfixed it, so the mass within the ligature was very slight on both sides and it was drawn very tightly. But for three or four hours after that woman was put to bed, such agony and suffering I do not think I have ever before witnessed. I gave her a quarter of a grain of morphia. She was vomiting, and two hours after the operation she vomited the characteristic coffee ground matter in large quantity. About 1 o'clock at night (the operation was done about noon), the pain became so intense I made up my mind I would not let the woman die of pain at any hazards, and I gave another quarter of a grain of morphia. It is true I had some difficulty getting the bowels moved; I gave about ten grains of calomel between the morphia, in all giving her some twenty-five grains of calomel, so that the bowels moved the next day after midnight. She had injections of strychnia, gr. 1-30, repeated every two or three hours, to relax the muscular tissue of the bowels.

A point of interest is that this is the third case within the last two years in which I have been compelled to use morphine. It has happened that in each one of these cases I believe I did just the right thing, for they all got well. This patient is entirely out of danger and is doing very well.

DISCUSSION OF DR. REAMY'S CASES.

Dr. Zinke:—Mr. President:—I did not come here this evening with the expectation of making any remarks, but when I listened to the case reports, especially the first one, I sometimes felt as if I must get up at once. Whatever I may have to say adverse to the procedure in that case, I certainly say with the greatest respect. But I do not believe, Mr. President, that the operation, which was done on that patient, has been unjustly forgotten. If we discover an intra-uterine polypus (I know this was not a polypus); even if the polypus be pedunculated and the pedicle be the size of the tumor itself—that is, a tumor which does not exceed in size perhaps a goose-egg or probably even an orange—enucleation per vaginam, with or without incision of the cervix—if we get sufficient dilatation without incision of the cervix, I believe it is best to do without—the operation is perfectly justifiable and is to be preferred to hysterectomy. But, Mr. President, if the procedure is generally adopted as was recommended to-night, where there is no pedicle at all and the uterine wall is so thin as the one-tenth of an inch, I am afraid that the same record of myomectomy that existed up to the last ten or fifteen years will be repeated. Many will die of hæmorrhage before the tumor is entirely enucleated.

Dr. Reamy: I would like the Doctor to understand that this case was worse than he seems to think. The tumor was without even a sessile pedicle; it was a typical interstitial tumor.

Dr. Zinke: I understand it just so, and all the more would I not countenance the procedure, notwithstanding it comes from such a distinguished authority. The majority of cases, I am sure, even if the Doctor himself continues to follow his course, will not be saved, and I think the Doctor is to be very much congratulated that he succeeded so well in this instance.

Dr. C. A. L. Reed:—Mr. President: I saw one of these operations performed by Dr. Richelieu in Paris, and while it was certainly done with great dexterity, I was very far from being impressed with its general application. It was the removal of a large diffuse myoma. In that instance there was a very considerable loss of blood, and I was very much interested in getting the after history of the patient. She had a pronounced septic manifestation about the second week after the enucleation, which had been carried quite as far as the attenuated walls would permit. Attention was called to the fact that there were distinct myomatous deposits that could not be dealt with because of the thin walls. Of course, this is only one exception to a rule. We have all been educated, as the speaker stated in his opening remarks, in the direction of abdominal hysterectomy, and perhaps we have gone a little far in that direction. Yet I would be a little reluctant, in view of the splendid results from abdominal hysterectomy, to abandon it for the French procedure, notwithstanding the dexterity with which they do it and the dexterity with which it was done in this case. In the meeting of the International Periodic Congress at Geneva, a gentleman made an assault upon the record of this operation, claiming priority for the very instruments referred to this evening; and, while his claim was made in the presence of Peon, Richelieu and others, his claim was not disputed.

Dr. Bonnifield:—Mr. President: It seems to me one of the strongest arguments against this operation is the fact that, as the essayist tells us himself, each time he manipulated the tumor before the operation, the manipulation was followed by sepsis is going to be of a mild character, as it was in this case, and that it will in a few days subside and leave the patient in condition to proceed with this operation by stages, is more than I can comprehend. The danger of the operation, of course, at the time, is hæmorrhage, but I was very much surprised at the time of this operation to see how little hæmorrhage there was. I think this depended on two things: First, the way he removed the tumor, that is, in rather small sections and rather slowly; and secondly, on the very fortunate circumstance that the tumor was not very vascular.

Dr. Reamy: They never are.

Dr. Bonnifield: Well, about a month or six weeks ago I had a patient from whom Dr. Reamy removed several fibroid polypi one or two years apart. She came to me with a submucous fibroid. My first impression was to remove the uterus, but when I asked one of my friends to see her, he thought the removal by morcelment would be less dangerous. I did so, but I shall never do so again. There was extreme hæmorrhage. Fortunately, I had ten or twelve yards of iodoform gauze at hand and managed to control the hæmorrhage. I think my judgment was bad in this case, for there were other fibroids present, and in such cases you can get the contraction of the muscular fibers of the uterus to stop the hæmorrhage. In Dr. Reamy's procedure he went slow enough to give the uterus time to readjust itself to the diminishing bulk of the tumor. I, unfortunately, tried to take one out a good deal quicker, and had a uterus that could not contract. As I said, one experience of that kind is enough for me.

Dr Edwin Ricketts:—Mr. President: A few years ago I reported to this society a case of a large fibroid polypus. Dr. Reamy said that sepsis should not surprise me, for with it we always have to deal in these cases. I never in all my experience had as many abscesses on the buttocks and along the thighs to open as in that case. It is true I saved my patient, but, as Mr. Tait used to say, "it was by the skin of her teeth." The last speaker on the floor expressed it very well when he said the doctor fortunately had a very mild case of sepsis to deal with in this instance. I do not think anything can be done to re-establish this procedure, of which Dr. Reamy has spoken, that was done over fifty years ago. Another thing, we often do not know whether we have one or more polypi or tumors to deal with, and then there are at times adhesions within the cavity, and it may be of the intestine itself. For instance, a writer of some distinction attempted to remove a tumor by morselmon in the presence of a friend of mine, and finally had to open the abdomen and release some adhesions before he could complete the operation. That patient came near dying on the table from hæmorrhage. Even if the exploratory incision is only large enough to admit the index finger, to see if the way is clear, it is of advantage. Although we do not have adhesions in all cases, still there are cases in which we do have them. I consider the operation morselmon a much more dangerous procedure than total hysterectomy per abdominal section.

Dr. A. W. Johnstone:—Mr. President: This wakes up to me what was a nightmare in my early gynæcological experience. I never saw more profound shock than the condition I used to see Goodell put his patients to bed in after this operation. Some died from hæmorrhage, and some died from sepsis. That was in 1883. In 1886 the only case I saw Tait lose from fibroid was in the preliminary step of splitting the capsule, as described here to-night. The patient promptly died of sepsis in less than a week. I have known of other cases in which the woman died, after such a procedure, with the whole thing gangrenous. You must remember, Mr. President, the vitality of this uterine tissue is very low and it will readily slough. After a long experience of watching other men do this operation, I am really glad that the procedure is almost obsolete. I believe we have heard to-night one of the echoes (although we must congratulate the gentleman) of an operation that is passing off the field. Where the tumor may be easily manipulated, it is much simpler, of course, to take it out. But where you have to burrow down and do not really know where you are going, I believe the procedure is absolutely dangerous and will never be recognized again.

Dr. Rufus B. Hall:—Gentlemen: First I want to congratulate the essayist on two facts: First, that he succeeded so nicely in enucleating this tumor, and, second, that his patient lived. I think these should be sources of great congratulation. My sentiments have been so graphically expressed by one or two speakers that I hardly need to refer to them. In this particular case, while the patient has recovered and still has her uterus and her ovaries, is 49 years of age, she has been subjected to a very much graver operation than an abdominal hysterectomy would have been, with a very much greater chance for dying from sepsis following the operation. Why should we subject her to such an operation to save her uterus and ovaries when she has no further use for them? Those are questions that came to me while listening to the report of the case and the discussion. I can appreciate how difficult it might be in a woman of fewer years, who was anxious to bear a child; I can understand how anxious any of us would be to gratify such a wish, and the patient might be willing to take an additional risk for the possibility of being granted the privilege of becoming a mother. But I cannot understand why we should do an operation which is more dangerous simply to save some useless organs. The operation must be done by separate sittings, splitting the capsule and taking away parts of the tumor to get ready for the final operation, and sepsis is, of course, what we would expect in such a case. Nobody can tell whether such a sepsis would be fatal or not. While I have many times removed intra-uterine polypi, I have never attempted an operation like this, and I am certain I never will on a patient that is near or past the menopause like this one. I will not say that I never will do the operation in a woman during the child-bearing period, because I believe under certain circumstances it would be justifiable to give the patient this operation. But there is another risk in an operation of this kind that no man can foretell, and that is hæmorrhage. We get the hæmorrhage from a fibroid, the uterus cannot contract, and the hæmorrhage may be severe or even fatal. No man could say there was only one tumor in this uterus before the operation. The operator could not assure the patient that she would not have another fibroid within three years and would have to undergo another operation. Therefore, I believe this operation will not come into general use and will be resorted to only on very rare occasions.

Dr. Zinke:—Mr. President: Do you think a uterus from which such a mass has been removed would be a safe uterus for pregnancy?

Answer: If that were the only tumor, I think it would be safe.

Dr. Ricketts: In nearly five out of six of these cases, the ovaries are in a diseased condition and the operator knows nothing of their condition. For that reason I wish to emphasize the advantage of an exploratory incision in diagnosis.

CLOSE OF DISCUSSION.

Dr. Reamy:—Mr. President: I have been really amused at the delicacy with which the gentlemen have introduced their criticisms, saying they meant nothing personal. I do not understand it. If I were a youngster, speaking here for the first time, it might excite me to have my methods opposed, or if I were advocating a method new and to be tried only at the risk of human life.

Dr. Hall: The reason I spoke in that way was because the operation has been defunct so long I thought an apology in order, because it was presented before the society.

Dr. Reamy: Intra-uterine polypi are not under discussion. I think nobody here would remove the uterus for an intra-uterine fibroid polypus. I take it for granted there are few here who would remove the uterus even if the attachment of the polypus were somewhat sessile. You remind me very much of the story of an Irishman employed to trim an orchard. When asked if he could trim apple trees, he said, "Yes, your Honor, I can." The Irishman was employed and in the evening the owner of the orchard returned and asked how he got along. Patrick replied that he got along very well. The man asked, "Did you get them all trimmed?" Patrick replied, "No, I haven't got them all cut down yet." And you, gentlemen, who propose to remove the uterus because it has a fibroma in it, remind me of the Irishman.

I will take up the criticisms in the order in which they were made. First, as to the question of sepsis. The cases to which my friend, Dr. Johnstone, referred, operated on by Goodell, were operated upon in 1883, when antiseptic surgery amounted to nothing.

Dr. Johnstone: As I saw them carried out, it was just as good as at the present day.

Dr. Reamy: Well, then, he was the only man, because at that time Lister had not formulated methods by which a sepsis could be carried on one-fourth as well as now. From what sources must the sepsis come in such an operation? My friend, Dr. Bonnifield, said I had a little sepsis when I made the preliminary incisions. I expected a little sepsis then, because it was in the vagina, and although the instruments were sterile and the parts were thoroughly sterilized before the operation was commenced, and although antiseptic treatment was continued, necessarily from the location there was a slight amount of sepsis; but I had no fear that sepsis would go to any considerable degree. These preliminary steps are only made in cases in which the lower segment of the uterus is involved in the growth. And what is the objection, suppose this sepsis should continue—an almost impossible thing in a properly selected case, for this was not a soft tumor? I would not subject a soft myoma or vascular tumor to such treatment. If you should find sepsis established itself in an individual and it was grave, what is to hinder a hysterectomy and the removal of all the source of sepsis? But I have never had sepsis in thirty cases, except in a solitary one, that amounted to anything, and in that one the sepsis was from the old practice of making the dilatation with tents. At the time that Dr. Johnstone refers to, Goodell, himself, in a large number of these cases, used tents in addition to a dilatation with instruments. This I have seen him do.

Dr. Johnstone: He did not use tents in the cases I saw.

Dr. Reamy: Goodell also used, in his cases, Goodell's divulsion forceps, and dragged these tumors away, and he recommended that procedure in his lectures. He did not attempt to cut them away.

Dr. Johnstone: Yes, he did, and I have seen him do it.

Dr. Reamy: He dragged them away. I know what I speak of, notwithstanding Dr. Johnstone's remarks to the contrary .Goodell left a large part of the tumor in the uterus. If you attempt to remove the tumor in that way in such a case as this, of couse you will destroy the woman's life. A man who is not accustomed to working in the uterus should not attempt this operation. But I beg you to remember that when you say it is a neat thing and easy and safer to remove the uterus than it is to remove only the diseased tissue, it is not the highest surgical science. The fact that a woman is 49 or 59 years of age is not against this. Your objections are purely theoretical. At the time Dr. Johnstone referred to, except by one solitary man, in proportion to the cases of abdominal hysterectomy, at that time more women died than died under Goodell from the removal of tumors by morselmon, as defective as it was. Because abdominal hysterectomy has been perfected and because advances have been made in vaginal hysterectomy, do you think it is, therefore, reviving a nightmare when we consider an operation that is more conservative, which leaves the uterus healthy and leaves the woman not mutilated? That is not science. Wait until you find out whether these women live or not; wait until you have tested whether the mortality from morselmon in properly selected cases is any greater than from hysterectomy, before you condemn it. Don't condemn it on theoretical grounds. I have lost only one case, and that was from sepsis induced before the operation. As to danger from hæmorrhage, I would not touch a soft myoma or any case which I could not dilate before the operation. In a large per cent. of these cases the operation can be finished in a single sitting. In conclusion, I would like to say this woman may have a recurrence; another tumor may grow. So may another cancer appear in the other breast after one breast has been removed, and so may a sarcoma in any part of the body after a sarcoma has been removed, and so may a patient die of tuberculosis after he has once recovered from tuberculosis; and should you cut out a woman's uterus because she might have a tumor in it some time in the future? I beg to say, without any personality, as you have said, the time is coming when not only myself but a large percentage of those present will be doing this operation. You will be compelled to do it, for the practicability of this operation will so impress not only you but the women themselves, who will demand it. The women will object to having you take away one of the constituents of their make-up when it is not necessary, and if you do not resort to this operation, gentlemen, you will not be as smart in this as you have been in the past. The blood supply of these tumors is, in almost every case, in the periphery of the tumor; and the reason this woman did not bleed was because it was a hard tumor and the larger vessels were cut off before I incised it. The uterus will contract and you can crowd it with gauze if necessary. But there is practically no danger from that nor from sepsis.

Dr. Johnstone: What is the mortality of this operation?

Dr. Reamy: Well, it depends very largely on the operator. During the last twenty years I have done the operation on at least thirty cases and have lost only one case, which was due to sepsis from the use of tents for dilatation.

Dr. Zinke: What was the nature of the case, which I saw with you, that died from hæmorrhage before we left the room?

Dr. Reamy: That was a case that died from shock, and has been reported by the doctor who had charge of the case.

Dr. Ambrose Johnson read the report of a case of fibroid tumor of the uterus.

Presentation of Specimen—Dr. Johnstone.

Mr. President: This is simply a large ovarian cyst and there were a great many adhesions, and some difficulties about the operation made it long and tedious. The reason I want to put it on record is because it is the first case of the kind I have come across. The patient is a woman aged 25, who has never menstruated, although she is thoroughly developed in every way. She has never been pregnant. Here is the opposite ovary, with a little dermoid. This is a proof of what Bland Sutton says, that these tumors are never found except in the Graafian follicle. You will observe that both the ovaries are diseased.

Dr. Palmer: It would be interesting to know whether, if this patient had been pregnant, she would have menstruated during the pregnancy. I had one case in which pregnancy has occurred three times, and the patient never menstruates until she is pregnant, and then she continues to menstruate during the period of gestation.

Dr. Reamy: I have two cases in which menstruation occurred only during pregnancy.

Dr. Johnstone: It would have been impossible for pregnancy to have occurred in her, because the Fallopian tubes were plugged.

Dr. Rufus B. Hall.

Case 1.—Hysterectomy for double pyosalpinx. Mrs. K., Troy, O., aged 30, married 12 years, 1 child 11 years old. Gave a history of specific infection. In November, '95, had an attack of inflammation, confining her to bed three weeks. At this time had a discharge of pus per rectum. March 1st, '96, was anæsthetized and the uterus curetted by her physician, Dr. Wright. She entered my private hospital Nov. 18, '96, and was operated upon the 21st. You will observe the specimen consists of both tubes and ovaries, with the uterus. Both tubes contained pus; there was an abscess in the left ovary holding an ounce or more of very offensive pus, and the right ovary contained about a dram. The adhesions in the pelvis were extensive. The fact that the uterus was almost denuded of peritoneum led me to select hysterectomy in connection with the removal of the suppurating tubes and ovaries, as offering the best chance for recovery. I feel convinced that in all cases of suppuration of both sides, the uterus being denuded of peritoneum, the best operation to make is hysterectomy. The patient has made an easy and satisfactory recovery.

Case 2.—Hysterectomy for multiple fibroma. The patient, Mrs. D., Glendale, O., aged 33, 2 children, youngest 9 years old, has been conscious of something wrong in her pelvis for three or four years. She suffered from profuse menstruation and severe pain at times in the abdomen. She entered the Presbyterian Hospital Nov. 26, '96, and was operated on the 30th. Both ovaries and tubes were bound down by adhesions.

The specimen consists of both ovaries and tubes, with the uterus, which is studded with multiple fibroma; the largest, size of a large orange, is in the posterior wall. This patient is also convalescing nicely.

Case Reports—Dr. Edwin Ricketts.

Mr. President and Gentlemen:—I want to report a couple of cases of rural surgery. In these days of antiseptic craze, it is well to go back and remember we can do surgery without these things when it is necessary. Not long ago I saw a case in which a woman had been tapped and relieved of 40 pounds twice and 10 pounds the last time, two weeks previous to when I saw her. I did not have a nurse and nothing but well water. I had a pitcher of this water boiled, and after cleansing my hands the abdomen was opened, and I found a multilocular cyst. The omentum had to be tied off in three segments. It was impossible to get the tumor out without flooding the cavity with fluid. I heated the water until it was comfortable to my hands and then I had to sponge the cavity from the pelvis to the diaphragm to get rid of the flakes. I found some flakes present from the previous tappings. Her reactive temperature was 100.4°, her pulse 110. The second day the bowels moved, the temperature was normal, pulse 84, and there was no distention or untoward symptom. She is now eating, and there is no doubt as to the result.

Second Case—Nearly seven weeks ago I saw a patient, æt. 68 years, who had been sick 90 days with abscess of the left lobe of the liver. The operation was done by cutting down into the pus, of which at least three pints was removed. In this case the drainage tube was discarded and iodoform gauze was packed into the wound. It has every indication of being a single abscess. The indications now are that we will have a recovery in this case.

THERAPEUTICS.

FORMULAE FOR EXTRACT OF FELIX MAS IN CHILDREN.

℞	Extr. filicis mar. eth	4.0
	Calomel	0.4
	Aquæ destill.	
	Sacchari albi	aa 15.0
	Gelatin	q.s.

M.—Sig.—To be taken twice or three times.

℞	Extr. filicis maris eth.	
	Tinct. vanilla	aa 3.0
	Syr. terebinth.	
	Aq. distill	aa 25.0
	Gum arabici pulv	2.0

M.—Sig.—One dose to be taken in milk.—Revue Intern de Médic et. Chir.—Pediatrics.

Hyoscine in the dose of 1-100 of a grain is of much value in the treatment of nocturnal emissions.—Hare.

Eclampsia never occurs without several days or weeks of premonitory symptoms. It is, therefore, to be considered as a perfectly preventable disease.—Davis.

In delayed union of bones, wiring is to be preferred to any other method of treatment in use at present. The wire may later be removed.—Brinton.

In the chronic form of cystitis, it is necessary to keep the phosphates in solution by rendering the urine acid. Boracic acid and benzoic acid, in doses of 10 to 15 grains, made into pill with a little glycerin, are of the greatest value.—Hare.

Eclampsia occurring before labor may often be benefited by hypodermic injections of morphine (gr. 1-3-1-2), and also of veratrum viride (6 to 20 drops of the tincture). The veratrum viride should keep the pulse about 60 per minute.—Parvin.

The most generally useful remedy in bronchitis is an ammonium mixture:

℞	Amonii chloridi	ʒ ij
	Ext. glycyrrhiz. fluid	ʒ ij
	Aquæ destill	℥ iij

M.—Sig.— ʒ j every four hours.—Hare.

The following is an excellent suppository for bleeding hæmorrhoids:

℞	Ferri subsulph	gr. iij
	Plumb. acet	gr. j
	Mass. hydrarg	gr. ss
	Ol. theobrom., q. s. ut ft. suppos. j.	

Introduce one morning and evening.—Horwitz.

In acute eczema the following wash may be used, allowing the sediment to coat over the parts:

℞	Calaminæ, Zinci oxidi	aa ʒ ij vel ʒ iij
	Glycerini, Alcoholis	aa ʒ j
	Liq. calcis	ʒ ij
	Aquæ	q. s. ad. ℥ ij

M.— —Stelwagon.

In fibroma uteri of small size, where the removal would be associated with danger to the structure of the uterus, Prof. Montgomery advises the use of five grains of thyroid extract three or four times daily, with a view to the absorption of the growth.

To remove comedo, or blackheads, use pressure, frequent washings with hot water and tincture of green soap, and the following stimulating lotion:

℞	Zinci sulphatis,	
	Potassii sulphureti	aa ʒ j
	Aq. rosæ	℥ iv

M.—If irritation is produced discontinue the prescription for a time.—Stelwagon.

When indicated, the following will be found useful as a sedative in first stage of labor:

℞	Hydrate of chloral	ʒ ijss
	Bromide of sodium,	
	Bromide of ammonium ....aa	ʒ ijss
	Syr. simp.	℥ ss
	Aquæ destill .... q. s. ad.	℥ ij

M.—Sign.—ʒ j every hour until three doses are taken.—Hare

—College and Clinical Record.

PERMANGANATE OF POTASSIUM AS AN ANTIDOTE TO MORPHIA.

Permanganate of potassium was first recommended by Barker Smith in 1877 as an antidote to morphia, experiments having shown that when taken after a large dose of either morphia or opium it may entirely neutralize the effect of these drugs, and it is also an efficient antidote against any vegetable poisons. Numerous cases have been reported in which morphia poisoning in children had been successfully treated by internal or hypodermic administration of permanganate of potassium. In a recent number of the Klinische Wochenschrift, Dr. Vucetic has published the case of a child aged 3 years who accidentally had taken about thirty drops of tincture of opium, and showed typical symptoms, viz., somnolence, cyanosis, contracted pupil and slow pulse. A 1 per cent. solution of permanganate of potassium was prescribed, and in the first place was injected with a Pravaz syringe into the right hypochondrium, in addition to which the child was given some teaspoonfuls of the solution. After some hours the toxic symptoms disappeared and the child eventually recovered, although it remained anæmic and showed some mental impairment. If it be considered that children up to the fifth year of age are extremely susceptible to morphia and opium, that those under four months sometimes show signs of poisoning after from two to four drops of laudanum, and that the legitimate dose of laudanum for children of from 2 to 4 years of age amounts to three and a half drops a day, it is clear that in the above case the recovery was due to the permanganate of potassium. The way in which the permanganate acts has not yet been cleared up, but experiments have shown that in a mixture of solutions of morphia and of permanganate of potassium the characteristic reactions of morphia are not obtainable. Only one case of poisoning with permanganate of potassium has been described; in this case, after a dose of about four ounces, death occurred from paralysis of the heart. In these cases Dr. Vucetic proposed morphia as an antidote. In morphia poisoning permanganate of potassium may be administered first hypodermically and then internally.—London Lancet.

MISCELLANEOUS.

The Roentgen Rays in the Diagnosis of Pulmonary Tuberculosis.—Prof. Ch. Bouchard.—At the last meeting of the Academy (The Medical Week, 1896, p. 607) I called attention to the fact that pleural effusion partly arrests the Röntgen rays, resulting in the formation of a dark spot, on radioscopic examination, which is in sharp contrast with the light color of the healthy side.

Pursuing my study of the cases of pleurisy referred to in this communication, I found that the light area marking the apex of the thorax became more extensive in proportion as the effusion was absorbed. In one of the patients, however, the opacity persisted at the apex, while a light space made its appearance toward the middle of the diseased side as the effusion decreased. When finally this effusion had been almost completely absorbed, the apex still remained dark. This fact, which had not been observed in the two other cases, led me to believe that there was condensation of the pulmonary tissue at the apex of the lung on the diseased side. Percussion and auscultation showed that this was the case and revealed the existence of incipient infiltration, masked at first by the plural effusion. Radioscopy had thus been the means of detecting pulmonary tuberculosis.

I then examined a number of tuberculous patients with the aid of the fluorescent screen, invariably finding that the pulmonary lesions threw a shadow, the limits of which corresponded exactly with the data furnished by the other methods of physical examination, the intensity of the shading indicating the depth of the lesion. In two cases, light spots, delineated on the dark ground, gave evidence of the existence of cavities, which was confirmed by auscultation. In other two cases, however, in which auscultation had revealed the existence of cavities, these did not show on radioscopic examination. In one patient, in whom constitutional symptoms and cough indicated incipient tuberculization, examination of the expectoration for the bacillus was negative, and the physical signs did not permit of arriving with certainty at the diagnosis of tuberculosis. Radioscopy showed that the apex of one lung was less permeable than the other, and a few days later all doubt was removed by the positive results of auscultation and bacteriological examination.

In diseases of the thorax, radioscopy furnishes information of equal value in every respect with that elicited by percussion. The air in the lung, through which the Röntgen rays pass, serves as a resonator for the percussion sounds. When this air is more or less completely driven out of the lung by an extravasated liquid or by morbid infiltration of the tissue, the penetrability of the thorax to the radioscopic rays decreases, resulting in greater or less obscurity, and at the same time the normal resonance decreases, being replaced by more or less complete dulness.—Medical Week.

AGGLOMERATING REACTION IN TYPHOID FEVER PATIENTS DURING INFECTION AND DURING IMMUNITY.—Drs. Widal and Sicard.—A comparative study of the agglomerating reaction during infection and during immunity could not very well be undertaken in acute experimental diseases. In the animals experimented upon, in fact, the limits of these two periods are difficult to determine. Human typhoid fever, by reason of its long duration and its exact cyclical character, is better than any other disease adapted to such studies during the febrile period and during convalescence.

The fact that the agglomerating reaction makes its appearance already in the course of the infection, sometimes even on the fourth or fifth day, has now been established by ample statistical evidence. The reaction sometimes decreases, and may even disappear, as we have found in two cases, during the first week of convalescence, at a time when immunity has been fully established. It again made its appearance during the apyretic period, a few days before recurrence, in patients who consequently did not enjoy immunity. The agglomerating reaction is usually absent or considerably attenuated in individuals in whom recovery from typhoid fever dates from upward of a year, and who are immunized, inasmuch as recurrence is exceptional among them.

We have examined the serum of twenty-two persons, in whom recovery from typhoid fever dated from one to twenty-six years. In only three of these, who had had typhoid fever respectively three, seven and nine years previously, we obtained immediate, marked reaction on mixing one drop of serum with ten drops of culture. All three had suffered from a severe type of typhoid fever of long duration, with recurrence. In three persons, in whom recovery from typhoid fever of medium severity dated from eighteen months, two and three years respectively, the reaction was slight and became perceptible only by mixing one drop of serum with five drops of culture.

Typhoid serum which agglomerates Eberth's bacillus does not appear to exert any bactericidal action on the microbe. We have kept for two months and a half, at the temperature of the laboratory, pure typhoid serums, inoculated with Eberth's bacillus, in which the agglomerations had not lost their vitality after this long time. The properties acquired by serum, frequently associated, may, therefore, sometimes be comparatively independent of each other.

Though there is yet no evidence by which to prove that the agglomerating reaction, like all changes brought about in the humors, is a means for the protection for the organism, it is probable that this is the case. If so, the clinical observations to which we have referred show that it constitutes a defensive reaction mainly during the period of infection.—The Medical Week.

SOME CLINICAL PECULIARITIES OF TYPHOID FEVER IN VERY YOUNG CHILDREN.—In the Annales de la Policlinique de Bordeaux for March there is a long and comprehensive article on this subject, by Dr. Rocaz, of which the following is the substance: Having had occasion to observe cases of typhoid fever in children under five years of age, at which time, all authors agree, the disease presents an appearance which is often peculiar, the author feels justified in adding his observations to those already published, and in drawing the following conclusions:

The onset of typhoid fever in young children is more frequently sudden than in adults, and its appearance is often marked by a sudden elevation of temperature, the thermometer rising in a few hours from normal to 104° F. and more. Rillet and Barthez think this sudden onset indicates an altogether peculiar gravity of the disease.

The intestinal symptoms are generally less marked in children than in adults. There is a rather exact relation between the number and extent of the ulcerations on the one hand and the age of the patients on the other hand. The younger the children the fewer and smaller the ulcerations. Constipation is the rule during the early days of the disease; it is obstinate and does not yield sometimes until after the administration of several purgatives. When it does continue during the course of the disease, it is replaced by a slight diarrhœa which presents nothing characteristic. Hence it may be readily understood that intestinal perforation and hæmorrhage are excessively rare in very young children. Enteritis, on the contrary, is a more frequent complication in children, and it is characterized by the persistence of the diarrhœa after defervescence; this diarrhœa is rebellious and sometimes requires an energetic treatment and a strict diet. Vomiting is frequent enough in children to be regarded as a part of the regular train of symptoms of the disease. Lack of appetite in young children is so pronounced as to become almost a veritable danger. M. Moussous has cited two cases in which the children refused everything, except a few mouthfuls of pure water, for twelve days.

With regard to the appearance of the tongue, the author states that in many cases observed by him in very young children, in which the temperature rose to 102.6° and 104.5° F., the tongue remained moist.

The temperature is very nearly like that observed in adults, although it often assumes a less regular progress in children. This irregularity has led to much discussion in regard to its prognostic value. The only fact that seems to the author worthy of mention, concerning the temperature in young children, is the very frequent occurrence of hyperthermia and the facility with which it is borne by the patient.

Dicrotism of the pulse has never been observed in very young children. During the height of the disease the pulse is generally regular, ranging from 100 to 140 pulsations a minute. During the defervescence and convalescence, however, it may present a peculiar characteristic which has been fully dwelt upon by de Gassicourt and Revilliot, who occasionally saw the pulse become slower and present irregularities which caused it to resemble the pulse in certain stages of meningitis. This peculiarity, which lasted from eight to fifteen days, did not seem to have any unfavorable significance. The irregularity of the pulse in the febrile period is more serious, for it indicates then a cardiac exhaustion which often presages death. The heart, however, is rarely seriously involved in typhoid fever in young children; myocarditis is exceptional and always slight, and sudden death has been observed only two or three times.

Thoracic symptoms are in the normal forms less marked than in adults, although broncho-pneumonia is a very grave complication in abdominal typhus in children under five years of age.

Lenticular rose-colored spots are the only pathognomonic symptom of typhoid fever in young children; they present no peculiar characteristic either in their frequency, their time of appearance, or their prognostic importance. Epistaxis is considered by the author as being altogether exceptional in young children. Sudamina are frequently observed, and they generally appear shortly after the red spots. Livid streaks near the articulations, particularly the knees, are peculiar to infancy, and they are due to an increase in growth which is sometimes surprisingly rapid. The frequency of desquamation is also a peculiarity of infancy.

With regard to the possible complications of typhoid fever in young children, the author mentions those of the nervous system, which, he says, are seldom observed in children under five years of age.

Other complications, such are as arthritis, otitis, thrush, torticollis, stomatitis, and phlegmasia alba dolens, are also frequent in very young children. Abnormal eruptions are more frequent in children than in adults.

The duration of the fever is shorter in infancy. According to some authors, relapses occur more frequently, but they are usually less serious than the first attack.

Convalescence is, therefore, rather rapid, and the child recovers with a rapidity that is sometimes astonishing. The aphasia of convalescence, which is very frequent in children, is nearly always curable.

The prognosis of typhoid fever in young children is dependent upon their age. It is very grave in children under three years of age, not so serious at four years, and nearly always favorable at five years and over.—N. Y. Medical Journal.

THE VALUE OF HOT SALINE IRRIGATION OF THE INTESTINE IN CASE OF URAEMIA.—In the American Medico-Surgical Bulletin of September 26, 1896, Grandin states that he has seen cases where, after delivery of the fœtus or after an abdominal operation, the kidneys refused to functionate, and where, with absolute certainty, uræmic symptoms would supervene unless the excretory organs could be persuaded to do their duty anew. Acute suppression of urine to a greater or less degree, headache, spots before the eyes, clouding of the intellect, twitching—symptoms always of such bad omen—such is the clinical picture he has in mind, and for which he recommends, above the administration of drugs, continuous irrigation of the bowel with hot normal saline solution. While the catharsis which is indicated is awaited, while the problematical effect of one or another drug is hoped for, the intestine may be irrigated, with the result of most profuse diaphoresis and, in the relief of the congestion of the kidneys, the betterment of all the alarming symptoms. In the cases under consideration, what we aim to secure is free catharsis and diaphoresis, with consequent abstraction from the circulation of the toxic elements which are at the bottom of the alarming symptomatology. The physician is dealing with a complication which must either be relieved speedily or, as a rule, eclampsia, coma, and death ensue.

To irrigate the bowel properly he proceeds as follows: The woman is placed in the left lateral position, with buttocks elevated and head lowered. A large rectal tube is inserted into the bowel as far as may be, usually up to the sigmoid flexure. The rectal tube is connected with a gravity syringe, which is hung at least six feet above the patient's head. In case such a syringe be not at hand, the physician will find a funnel in every household, and this may be connected with the rectal tube by means of rubber tubing. Hot salt water is used for irrigation. The strength of the solution should be about one per cent., and the temperature of the water in the receiver about 188° F. An attendant should hold the rectal tube at the anal margin to prevent its being expelled as, under the provoked peristalsis, the water is driven out of the bowel. From eight to ten gallons of water should be allowed to flow in. This accomplished, the woman should be wrapped in blankets and made comfortable in her bed. Meantime croton oil may be placed on the tongue, and glonoin may be administered in full doses hypodermically in the event of the character of the pulse demanding it. It may be stated here that, as a rule, in the condition under consideration, glonoin is called for, but the dosage must be large—that is to say, fully one-twenty-fifth of a grain, repeated half-hourly until the physiological effect has been secured. This drug offers us the the readiest of all means for relaxing the spasm of the renal capillaries.

Very soon after the irrigation, profuse diaphoresis sets in, followed by abatement in the alarming symptoms. and shortly thereafter the kidneys may begin to excrete again.

The explanation of the effect of hot saline rectal irrigation is not far to seek. The nerve centers are stimulated; the kidneys are direclty stimulated; the skin is called into action; peristalis of the intestinal tract is evoked. In short, every indication is promptly met.—Therapeutic Gazette.

INGUINAL HERNIA CONTAINING A PREGNANT FALLOPIAN TUBE.—Jordan (Münchener Medicinische Wochenschrift, No. 1, 1897), reports the following case from Czerny's clinic in Heidelberg: The woman had undergone several operations for pyosalpinx and vesico-vaginal fistula. The pyosalpinx had ruptured into the right broad ligament, the pus finding its way under Poupart's ligament where it had been evacuated, leaving a scar. The patient had been seized with severe abdominal pain and repeated vomiting, and suspecting pregnancy had visited the clinic for examination. Upon admission she appeared very anæmic with a pinched face and rapid pulse. Above the scar in her right side was a swelling as large as a fist and tympanitic on percussion. Below the scar was another tumor smaller in size, soft, and non-reducible. A third swelling was found to the outer angle of the scar corresponding in position to the internal inguinal ring. This was the size of an apple, soft, irreducible, and painful on palpation. High rectal enemas were administered and resulted in neither stool nor flatus. Purgatives were not retained. Operation was decided upon two days after admission on account of increasing severity of the symptoms of intestinal obstruction. An incision was made parallel to Poupart's ligament showing a hernial sac containing a large mass of blood-clots, a fœtus about three months old, the uterine adnexa, and coils of intestine. The wound was cleaned as much as possible and tamponned with gauze. The vomiting continued and death occurred the following day. It was found by autopsy that the intestinal obstruction was caused by numerous adhesions which were beyond surgical relief.—Univ. Med. Mag.

An Unusual Case of Lockjaw.—Sachse reported in the Centralbl. f. Chir., No. 40, 1896, an unusual case of lockjaw, occurring without known cause in a male, aged 24 years. On waking one morning he found he could not open his mouth as far as usual. The disability increased daily, until he was unable to separate his teeth. For four years he was treated without benefit, until it was discovered that the right upper wisdom tooth pointed directly outward, and so pressed against the internal pterygoid muscle as to prevent the jaw from opening. It was removed with difficulty. Immediately after its extraction the jaw could be partially opened, and in eight weeks function was fully restored.

Fibrinuria.—Arthur Klein (Wien. Klin. Woch.) gives the history of a case in which fibrinous clots were passed, and analyzes the previously recorded instances (4 in number) in which this has occurred. The patient was a man aged 52 years, who had been ill for fifteen months with cough, headache and dyspnœa on exertion, to which had recently been added swelling of the extremities and rigors. Examination led to the diagnosis of Bright's disease; the urine was acid, of specific gravity 1,013, and contained a large quantity of albumin, with numerous hyaline and epithelial casts. Treatment consisted in hot baths and milk diet, and the patient improved considerably, the œdema almost completely disappearing. Ten days after admission it was noticed that the urine, which was slightly alkaline, contained a large number of grayish white clots, some rounded and others flattened, and varying in length from half an inch to four inches; some were as much as two inches thick. These, if allowed to stand, settled to the bottom, and the supernatant urine sometimes showed a further curious phenomenon in the shape of fine threads, which formed a network extending throughout the fluid. On shaking, this became detached from the sides of the vessel, and formed a well-marked clot; the urine had thus clotted just like a serous fluid. The amount of deposit remained constant except just before a rigor, when it diminished, to return to its former amount with the subsidence of the temperature. If the clot was allowed to remain in the urine for a few hours it disappeared entirely, probably by a sort of process of self-digestion. Chemical examination proved the clot to be composed of fibrin. The urine from which it was deposited showed, as compared with that at other times, alkalinity, much diminution in solid contents, an especially minute amount of phosphatet, but an enormous quantity of albumin.

There could be no doubt as to the renal origin of the fibrin, but it was very hard to define its pathological cause; evidence of nephrolithiasis, kidney abscess, etc., which had been present in former cases were here wanting, nor did hematuria ever appear. Klein attributes the coagulation to the alkaline reaction, the large quantity of albumin, and the almost complete absence of phosphates; it bore also, no doubt, some relation to the sudden rigors and rises of temperature.

Some time after the completion of the experiments the patient became worse and died; the kidneys were found to be in a condition of amyloid degeneration, and their tubules contained hyaline masses and threads giving Weigert's reaction for fibrin. Klein adds the notes of analyses of two other kinds of clots. The one was found in the urine in a case of cystitis; it consisted of a capsule of mucin. The other was a cast from a patient suffering from plastic bronchitis, and this failed to give the fibrin reaction either in bulk or in section; it consisted of mucin, and contained large colonies of diplococci.—Br. Medical Journal.

Professor Kocher, of Berne, has the most remarkable record of the world's history in the extirpation of goitre. In a late summary of his work he said that during the past ten years, in which he has performed the operation on nine hundred patients, he has met but one case in which cachexia strumipriva developed. This was entirely owing to the fact that he always leaves a small part of the thyroid gland, which is sufficient to carry on the functions of that organ. In the single case in which the cachexia developed, the extirpation was unilateral, but after the operation it was found that the other side was atrophied; the patient recovered through thyroid feeding. In speaking of the mortality in his operations, Kocher says that he deducts thirty cases of malignant goitres in which unusual and peculiar difficulties militated against success. Of the remaining 870, eleven died, but in six only was death the direct result of the operation, and these three were operated on for Graves' disease. The extirpation of the goitre in the latter disease he regards as dangerous. For the latter he prefers to ligate the thyroid arteries, but never more than three of them.

In a report entitled "The Use of Antitoxic Serum in the Treatment of Diphtheria, Under the Supervision of the New York City Health Department," by Drs. Biggs and Guerard, the following are given as the conclusions:

We desire to bring out strongly and clearly the fact that it matters not from what point of view the subject is regarded, if the evidence now at command is properly weighed, but one conclusion is or can be reached, whether we consider the percentage of mortality from diphtheria and croup in cities as a whole, or in hospitals, or in private practice; or whether we take the absolute mortality for all the cities of Germany whose population is over 15,000, and all the cities of France whose population is over 20,000 (in France and Germany antitoxin has been more generally employed than elsewhere); or the absolute mortality for New York City, or for the great hospitals in France, Germany, and Austria; or whether we consider only the most fatal cases of diphtheria—the laryngeal and operative cases; or whether we study the question with relation to the day of the disease on which treatment is commenced, or the age of the patient treated; it matters not how the subject is regarded or how it is turned for the purpose of comparison with previous results, the conclusion reached is always the same, namely, there has been an average reduction of mortality from the use of antitoxin in the treatment of diphtheria of not less than fifty per cent., and under the most favorable conditions a reduction to one-quarter, or even less, of the previous deathrate. This has occurred not in one city at one particular time, but in many cities, in different countries, at different seasons of the year, and always in conjunction with the introduction of antitoxic serum, and proportionate to the extent of its use.

Then, finally, there is to be added not only the experimental proof of the specific action of antitoxin in neutralizing the toxin of diphtheria, but also the overwhelming mass of evidence derived from the personal observations of the most distinguished practitioners of medicine of every country. There are to-day, in the whole civilized world not more than three or four active opponents of the antitoxin treatment of diphtheria, whose names were known to the medical profession before the introduction of antitoxin.

It is well to bear in mind the fact, that what is called conservatism in medicine is often only a cloak assumed to cover up indolence. The acceptance of a new principle, or a new fact, involves the expenditure of a certain amount of mental energy. Old ideas and conceptions must be discarded or readjusted to the new information acquired. This involves labor and effort, which some are not willing to expend. They are not willing to go over the data at hand and form conclusions for themselves, but shake their heads wisely, predict a reaction, and plead for conservatism. It is not possible now for any unprejudiced person to master the evidence available regarding the value of antitoxin in the treatment of diphtheria, and reach anything but a positive conclusion.

The deductions to be drawn from a review of these statistics and reports presented would seem to be self-evident and conclusive. The value of antitoxin in diphtheria is no longer a question of opinion or theory, but an established fact. "Probiren geht über studiron," those who have had the largest experience with the remedy and have most thoroughly tested its merits, are most in favor of it. The few who oppose it have proved nothing in comparison with the enormous mass of evidence as to its specific value.

It may, therefore, be affirmed that the following facts have been demonstrated:

1. That diphtheria antitoxin, where generally employed, has reduced the mortality from diphtheria at least one-half.

2. That it has a distinctly favorable effect on the clinical course of the disease, shortening it and lessening its severity.

3. That the earlier the treatment is commenced the better the results obtained; the mortality, when adequate doses of antitoxin have been given with the first forty-eight hours of the disease, not exceeding five per cent.

4. That antitoxin is a specific against true diphtheria (*i. e.*, where the symptoms are due solely to the Klebs-Loeffler bacillus) and is less efficacious in mixed infections, but even in these forms of diphtheria is of decided benefit.

5. That it is not necessary to wait for a confirmatory bacteriological diagnosis, but that in every clinically suspicious case of membranous angina, especially in children, a medium dose of antitoxin should immediately be given, and repeated if required by the further developments of the case.

6. That antitoxic serum is a remedy without serious after-effects in the doses which have ordinarily been employed (the after-effects, such as rashes, etc., being insignificant in comparison with the danger of the disease); that it has no injurious action on the kidneys, the heart, or the nervous system; that it does not entirely prevent albuminuria, heart failure, and post-diphtheritic paralysis, because the effects of the diphtheritic toxin which has already entered the system before the administration of the remedy, no matter how soon the treatment is begun, are not always completely counteracted by the antitoxin, though there is every reason to believe that in sufficient doses it does prevent any farther extension of the toxic action after its effects have been produced.

7. That the protection conferred by immunizing doses of antitoxin

is almost absolute for a short period of time, *e. g.*, three or four weeks, when a sufficient number of antitoxin units is administered, and that with a high grade preparation, where only small quantities of serum are required, the remedy is absolutely harmless even with the youngest infants.

8. That antitoxin, if not a specific cure for all forms of diphtheria occurring in the human subject, is by far the best remedy for the treatment of the disease.

To the critics of the antitoxin treatment I may repeat the words of a famous German poet, quoted by Professor Soltman:

"Das ist die richtigste Kritik von der Welt,
Wenn neben das was ihm missfällt,
Einer etwas Ergens and Besseres stellt."

Or, in other words:

"The best critics in the world are they
Who along with that which they gainsay,
Suggest another and a better way."

:o:

NEWS AND NOTES.

IMPORTANT ANNOUNCEMENT.

Meeting of the American Medical Association.

To the Editor of the Philadelphia Polyclinic:

Dear Sir:—May I call the attention of those of your readers who are thinking of coming to Philadelphia to attend the semi-centennial meeting of the American Medical Association, to be held June 1, 2, 3, and 4, 1897, that there will be, in addition to the American Medical Association, at the same time a large mercantile convention which will to a certain extent utilize a considerable number of rooms in the various hotels.

For this reason it is advisable that physicians who intend to be present at the meeting should write at once to one of the following hotels, engaging such rooms as they desire at the rates named:

Hotel Walton, Broad and Locust streets: $1.50 and upwards per day, European plan; $4 and upwards per day, American plan.

The Colonnade, Fifteenth and Chestnut streets: $1 and upwards per day, European plan; $3 and upwards per day, American plan .

The Lafayette, Broad and Chestnut streets: $1 and upwards per day, European plan; Table d'Hôte: breakfast, 25 cents to $1; luncheon, 75 cents; dinner, $1.25.

The Bingham House, Eleventh and Market streets: $2.25 and upwards per day, strictly on the American plan.

Hotel Stanton, Broad and Spruce streets: $2 and upwards per day, European plan; $4 and upwards per day, American plan.

The Continental, Ninth and Chestnut streets: $3 and upwards per day, strictly on the American plan.

The Windsor, Eleventh and Filbert streets: $1 and upwards per day, European plan; $2 per day, American plan.

The Stratford, Broad and Walnut streets: $1 and upwards per day, European only.

Girard House, Ninth and Chestnut streets: $2.25 to $3 per day, strictly on the American plan.

Hotel Hanover, Twelfth and Arch streets: $2.50 per day, strictly on the American plan.

Aldine Hotel, Chestnut street above Nineteenth: special rates to members of the American Medical Association, $2.50 per day on American plan; $1 to $3 on the European plan.

The price quoted in each instance is for one person only. Rooms commanding only the lowest price are naturally limited in number. It is especially desirable that each member intending to be present at the meeting shall personally, or by letter, make his arrangement with the hotel at which he desires to stop.

It is worthy of note that the rate of $1.50 per day at the hotel headquarters, the "Hotel Walton," is the rate for two people in one room. All these hotels are within a few blocks of the meeting places, and most of them are within two blocks.

As a sub-committee of the general Committee of Arrangements has arranged clinical courses in all branches of medicine at the various teaching institutions and large hospitals during the week prior and following the week of the Association meeting, it has been thought that a considerable number of physicians would be glad to embrace the opportunity of brushing up upon the various branches by attendance on these courses, for which no charge will be made by the gentlemen giving them, and as their stay in this city will therefore be more than a few days, it has occurred to the Committee that some of the visiting physicians may wish to take rooms at some good boarding-house. The Chairman of the Committee on Reception and Accommodation, Dr. G. E. de Schweinitz, 1401 Locust street, will be glad to send the addresses of such boarding-houses to gentlemen desiring to stay here for a week or more.

The large number of gentlemen who have already signified their intention of attending the meeting, the very large number of able and interesting papers already placed upon the programme indicate that this will be the most important meeting which the Association has ever had, and it is hoped that every physician who is a member of a regularly organized County Medical Society will make an effort to attend.

The meeting halls for the various sections are situated so close to one another that different papers in different sections can be readily listened to during a single morning's session by those who do not wish to devote their time to one particular specialty.

Very truly yours,
H. A. Hare, M.D.,
Chairman of the Committee of Arrangements,
American Medical Association.

REVIEWS.

Surgical Hints for the Surgeon and General Practitioner. By Howard Lilienthal, M.D., Assistant Attending Surgeon to Mt. Sinai Hospital, New York City, New York. International Journal of Surgery Company, 1897. Price 25 cents.

In writing this little book the author's aim has been to present a number of observations and suggestions whose value has been thoroughly tested at the bedside and in the operating room. A review of its pages will show how much practical information he has conveyed within a small compass, and this he has been able to do by eschewing all superfluous verbiage and by writing clearly and to the point. The material is well arranged, the typography excellent, and the little volume is of a convenient size to be carried in the pocket and perused at leisure moments.

Autoscopy of the Larynx and the Trachea. (Direct Examination Without Mirror.) By Alfred Kirstein, M.D., Berlin. Authorized translation (altered, enlarged, and revised by the author) by Max Thorner, A.M., M.D., Cincinnati, O., Professor of Clinical Laryngology and Otology, Cincinnati College of Medicine and Surgery; Laryngologist and Aurist, Cincinnati Hospital, etc. With twelve illustrations. One volume, crown octavo, pages xi-68. Extra cloth, 75 cents, net. The F. A. Davis Co., Publishers, 1914 and 1916 Cherry street, Philadelphia; 117 W. Forty-second street, New York; 9 Lakeside Building, Chicago.

This little book on autoscopy is brief, clear, and sufficiently illustrated to put the subject within the grasp of every practitioner. While such a simple method of examination has been always within such an easy reach, it is surprising that it should have been so long before it was elaborated. The author, however, wants it distinctly understood that he has no prejudice towards the laryngoscopic method, that his own method does not render superfluous the laryngeal mirror, but simply adds to it.

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EDITORIALS.

MEDICINE AS A BUSINESS.

The traditions of medicine have become so firmly engrafted upon the lives and methods of the physician, that he awakes as from a dream when confronted with the consideration of the business aspect of his calling. In vain does the man who pursues the study and practice and medicine in its true spirit, endeavor to escape from the mercenary consideration of dollars and cents. Like Hamlet's ghost, "it will not down," because his daily wants and the duties to his family demand it persistently and loudly.

It is idle and Utopian to regard medicine as an ideal calling which we have entered and which we pursue for the good of suffering humanity. We must face the problem presented by "Medicine as a Business." Much ink and paper have been wasted in the effort to reconcile the humane with the mercenary element of medicine. The nearest approach we can make, as conscientious men and true physicians, is to adopt the golden rule, and this suffices amply to guide us in the path of true science, to cheer us in the more difficult labor of true humanity, and to avoid the pitfalls of mercenary action.

It is meet, indeed it is needful that while the doctor charges and collects his fees, he should be more considerate of the poor and needy than are all others; that while he attends strictly to his own affairs, he avoids interference with those of his brother physician. To the latter we desire chiefly to direct our remarks.

The discussion at a recent meeting of the County Society on the subject of "Free Dispensaries," emphasizes the necessity of regarding our brother practitioner's interest with a little more concern than had been our want. Those who are so fortunate (?) as to be on the staff of hospitals and dispensaries, know too well how often they are called upon to treat patients who are far more prosperous than some of the physicians who give them gratuitous services. They know, too, that in many instances their labor is not worthily bestowed. They know, too, that often the recipient could and would have sought out a struggling colleague in his neighborhood if the way to the dispensary were not wide open inviting him or her to the deception by which the institution is defrauded, and two doctors are cheated.

It would seem that there can be but one opinion on this subject. The dispensaries at least should be limited by the law. While we may agree with the able opponent of the law which has fallen by the way, that the withdrawal of the State's authorization (incorporation) should come from a higher tribunal than the Charity Organization Society, we would welcome any change which would temporarily remove this crying need of multiplication of dispensaries. The cartoon of the N. Y. Herald of May 25, is an eloquent exposition of this whole matter, depicting a haggard, careworn, spectacled doctor, with a placard over his sunken chest, "Please help the poor young physician," while on his right the door of the "Free Dispensary" is filled with crowds eager to enter it, and on his left the elegant portals of a hospital with 10,000,000 free beds, invites the lame and the halt to partake of its hospitality, its skilful treatment, and its good food and nursing.

The way of the young physician is indeed hard when he is ground between these upper and lower millstones, and it is our duty to remove this obstacle to the financial welfare of our calling.

The law permitting dispensaries to the actually poor will do much to remedy this crying need. But when will our benevolent people cease to erect grand hospitals, adorned with marble and fine stone, with turrets and artistic cupolas, only to ask for more money to complete them, while the wards are not as full as the wards of less pretentious hospitals? So long as the deserving poor are treated in these hospitals, there is no cause to complain. But when well-to-do people are sent there by advice of the consulting physician and surgeon who are called by the hard-working doctor, they remain there as private patients of the former, and the family doctor goes on seeking other cases to distribute among his brilliant confreres.

People of means commit a great error in leaving their comfortable homes in the hope of obtaining better care in the hospital. We say this to the family physician who generously and unselfishly sacrifices his own best interests to what he deems the patient's welfare. His patient does not receive better care in the hospital, as we know from personal observations. The great doctor having fifty or more patients, and perhaps a half-dozen private patients to visit in the brief hour allotted to his hospital visit, frequently glances at his watch in order to neglect a promised consultation. He cannot and does not give the private hospital patient the same time and attention which the latter would receive at home, because of the large amount of work much of it perhaps unexpected, with which he is besieged. The house physician's history is often accepted, his diagnosis hurriedly verified, and the treatment is prescribed. The great surgeon who has persuaded the family doctor that this case of appendicitis or what not would be better cared for, errs too. We have seen this surgeon rushing from one private room to the other the patient's wound sometimes exposed until he finds it convenient to reach him. The dressings are made by the house surgeon and the case is then dismissed, while at the residence of the patient the dressings are removed by the nurse, but the surgeon dresses the wound himself, and becomes better acquainted with its condition, because he has nothing to divert his attention. He has allotted a certain time for this case and is not confronted by new cases and emergencies which demand care and which deprive the private patients of the deliberate and painstaking examination and dressing which his case demands. The claim is often made, too, that the hospital offers opportunities for more perfect asepsis than a private residence. This is not true. A surgeon who is an adept in asepsis, has hospital assistants and nurses who are painstaking and cautious; such a surgeon will also have in private practice assistants and nurses who follow the master in every detail because his watchful eye is upon them. Since sepsis from air is no longer the bugaboo of surgeons, the ordinary thorough cleansing of the best room in a flat or house may be made sufficiently aseptic. The poor laborer and the hard-working clerk or salesman will doubtless profit by care and attention they would receive in hospital, which their means would not admit at home. But the man or woman who has the means to obtain the necessary nurses, will fare far better as a private patient at home than as a private patient in a hospital.

It is quite different in the matter of dispensaries. The very poor cannot pay the physician and druggist, and must resort to dispensaries. But the clerk, or salesman, or small merchant whose ailment does not prevent his attending to his duties, saves time and money by consulting his own physician, who charges from 50 cents to $1.00. He must spend a whole afternoon or morning in crowds which are jostling in the waiting room of the dispensary, he must pay something for his medicine, and he must wait two days to ascertain if a change be necessary in treatment. If it be an eye or ear case, a wound requiring dressing, he will surely do better to have a conscientious young physician, perhaps the same who attends the dispensary, to give him careful deliberate attention, than to be hurriedly treated, as he needs must be, owing to the crowds.

These things will right themselves if the law will prevent the incorporation of dispensaries for private gains, and the public be informed on the subject.

THE ABUSE OF MEDICAL CHARITY.

The committee appointed by the Society of the County of New York—the largest county medical organization in this country—to look into the question of the abuse of medical charity, made its first report on Monday, May 24, 1897. As might have been expected the report called forth a spirited debate. The fact was brought out that one dispensary in this city had made ten thousand dollars or more by its so-called charitable work; and that another received a not inconsiderable revenue from its poor patients. Other figures were given to show the absurd extent to which free treatment had been carried. The committee first sent a circular letter to all of the dispensaries in the city in which these three questions were asked:

First—Does the Governing Board of the dispensary approve of the movement to abolish or regulate the abuse of medical charity?

Second—Will the Governing Board co-operate with the committee of the Medical Society of the County of New York and endeavor to dispense free medical treatment only to those applicants that are deserving and unable to pay a physician for such services?

Third—Will the Governing Board agree, at such time as called upon, to display in conspicuous places in the dispensary a placard reading somewhat as follows:—"On and after ——— the case of every patient applying for free medical treatment at this dispensary will be investigated, to determine if the applicant is deserving of free medical service," providing such agreement is not to be construed as binding the dispensary to make such investigations on its own account?

An early reply is requested.

Replies were received from seventy-six out of the ninety-five dispensaries addressed, and all were affirmative except those received from the Dispensary of St. Mary's Free Hospital for Children and the outpatient department of the N. Y. Hospital, New York.

A decided step towards the remedy of the evil had been taken by the co-operation of several societies and the State Board of Charities in the passage unanimously by both Senate and Assembly of the bill by Mr. Sullivan entitled the "Dispensaries Bill," which was as follows:

"SECTION 1.—By this act a dispensary is defined to be any institution, agency or place, society or association, whose actual or alleged purpose it is to furnish gratuitously or at a merely nominal price to indigent, needy or other persons not resident therein, medical or surgical relief, advice or treatment, medicine or orthopædic or other appliances.

"SEC. 2.—On or after the first day of October, 1897, it shall not be lawful for any one to establish, conduct or manage at any place in this State a dispensary not duly incorporated as such under the laws of this State, or not connected with another corporation and licensed by the State Board of Charities.

"SEC. 3.—In no case shall a dispensary be established, carried on or conducted in any place in this State commonly known as a drug store, nor in any place or building in the State defined by law or by an ordinance of a board of health as a tenement house.

"SEC. 4.—It shall not be lawful for any person or persons to display the word 'dispensary' or to cause the same to be published in any form or in any manner in order to attract any indigent, needy or other person to any dispensary not duly incorporated or licensed as provided in section 2 of this act.

PENALTY FOR VIOLATING THE LAW.

"SEC. 5.—Any person who shall by means of any wilful false representations on his or her part obtain at any dispensary medical or surgical relief, advice or treatment, medicines or othopædic or other appliances, or any person who shall wilfully violate any of the provisions by this act shall be guilty of a misdemeanor, and upon conviction shall be required to pay a fine of not less than fifty nor more than two hundred and fifty dollars.

"SEC. 6.—The State Board of Charities is hereby empowered to make rules and regulations and to alter and amend the same, when in its opinion necessary, in accordance with which indigent, needy or other persons shall be given medical or surgical relief, advice or treatment, medicines and orthopædic or other like appliances by such duly incorporated or licensed dispensaries; and the said Board is empowered, a chance for a hearing having been given, to annul the incorporation, or suspend the operations or to revoke the license of any dispensary for wilful neglect or failure on the part of its managers, trustees, officers or employes to comply with the rules and regulations so established by said Board; but nothing in this act contained shall be considered to mean that said Board shall have power to determine the particular school of medicine under which the dispensary shall be conducted.

"SEC. 7.—All acts or parts of acts inconsistent with the provisions of this act are hereby repealed.

"SEC. 8.—This act shall take effect on the first day of October, 1897."

While this is, perhaps, not all that can be desired in the matter, it received the enthusiastic support of the proprietors, and to the great disappointment and chagrin of those interested in reform in these matters was defeated by the Governor's failure to sign the same. It is fairly well known that special personal influence was brought to bear to defeat the measure after it was given to the Governor, and the day of reckoning may yet come. The fact that so much power was given by it to the State Board of Charities, unquestionably was also a material factor in the prevention of its becoming a law. However, the wheels of progress are in motion; the fact that medical men themselves cannot or will not do all that is necessary was long ago apparent; and the hope that by legislation we are sometime to be rid of at least some of the objectionable features of the situation is stronger than ever. The committee was continued; and it is not too much to expect that another twelve months will show a marked advance in the abatement of the evil.

THE BACTERIOLOGY OF BALDNESS.

Our friend whose hair is rapidly failing will be interested in the following from our British cousin:

Sabouraud's discovery that there are varieties of ringworm caused by a plurality of fungi was only recently made, and before we have had time thoroughly to grasp the bearing of this important gain to our knowledge, another discovery by this indefatigable investigator is brought before the scientific world. That seborrhœa is invariably associated with a specific microbe is a fact which is not only new to us, but which was probably scarcely suspected. That this microbe should not only be found in seborrhœa, but that it should also be found in alopecia areata adds to the surprise with which these announcements must be received.

The facts on which they are founded are explained in some detail in a paper by Dr. Wickham. M. Sabouraud tells us that if we scrape from the skin the oily exudation obtained by pressure from a part affected by seborrhœa, and spread it on a coverglass, after dissolving the fat by ether, and coloring for five minutes in gentian violet, and then decolorizing by Gram's solution, alcohol and aniline oil, we can detect myriads of a special microbe which is a very fine bacillus. This bacillus when young is punctiform and almost resembles a coccus, having, after coloration by Gram's method, a length of less than 1 μ. The adult long by 0.5 μ forms are 1 μ in diameter. The sigmoid forms, which are in short chains, are rare in the exudation. They may reach the length of the tubercle bacillus. This microbe is the bacillus of seborrhœa, and it is found, as Dr. Wickham explains, in rounded masses in the upper third of the hair follicle. When to this bacillus of seborrhœa are added other organisms, which is often the case, we get secondary affections of the hair follicles—various forms of acne. The same bacillus affects the sebaceous glands of the scalp, where it produces depilation or baldness. The hair of the affected follicle dies, and is seen, when examined under the microscope, to be normal in its oldest part, and atrophied in its youngest. The scales and crusts that are formed on a seborrhœal head are due to the superposition of other microbes, but here they do not as a rule cause acne.

The mechanism by which the seborrhœa bacillus produces baldness is not understood. Anatomically it leads to progressive hypertrophy of the sebaceous glands and an exudation of leucocytes around the papilla of the hair. After this condition is established the hair falls, and each new hair that takes the place of one that is lost is weaker and smaller than its predecessor.

Inoculations in animals with this microbe present great difficulties. That is why Sabouraud states in his paper in the Annales de l'Institut Pasteur, "I have been able to state that the micro-bacillus of seborrhœa, unique in kind, and innumerable in amount in the lesions, is the constant microbic feature of this affection, without nevertheless being able to prove that it is its cause, since I have not been able to reproduce with it at will the type of the disease in which it is met."

From seborrhœa to alopecia areata there is, clinically, a wide gulf, which is, however, aparently bridged by Sabouraud's investigations. The essential difference between the hair affected with ringworm and the hair affected by the disease which causes alopecia areata is this. In ringworm the hair papillæ manufacture the hair in the ordinary fashion, but as soon as it is made the ringworm fungus destroys it, whilst in alopecia areata the fall of the hair is caused by a suspension of the formative power of the hair papilla. A similar state of things is the cause of the loss of hair in seborrhœal baldness. The atrophied hairs of alopecia areata differ very slightly to the naked eye from those which are killed by seborrhœa. The process is essentially identical, there being a difference only "in time and in place and in degree." In seborrhœa the depilation is chronic, incomplete, and diffuse, and its seat of predilection is the vertex. In alopecia areata the baldness is complete, localized, and may occur at any part. A patch of alopecia areata is an acute local affection of seborrhœa. This is proved by the fact that if a section is made through the skin affected by alopecia areata in an early stage, the hair follicles, without exception, are found to be infected by the micro-bacillus of seborrhœa, whilst around the affected surfaces the scalp is healthy, and the follicles are not affected. Sabouraud has found this condition present in all the cases he has examined, sections of no fewer than thirty-two portions of skin having been investigated, and a whole year given up to that special work. Up to the present time, he tells us in a footnote, he has obtained in the sheep, the guinea-pig, and the rabbit, characteristic patches of alopecia areata by using cultures of its microbe. For him, therefore, seborrhœa and alopecia areata are "essentially identical." The patch of alopecia areata is only an attack of acute circinated seborrhœa, and inversely the bald only become bald by a diffused process of chronic alopecia areata. "I understand well," he remarks, "how much this opinion will appear anarchic and monstrous to dermatologists, and I believe will be received by them with incredulity, but so did the statement that a boil and osteomyelitis are caused by the same organism meet with incredulity. The statement of every new fact is thus met."

It has not been our object, in giving this brief summary of Sabouraud's researches, to enter at all into the question of previous investigations regarding the nature of seborrhœa and alopecia as areata, but it is interesting to observe that Unna had, as Sabouraud himself states, seen in seborrhœa of the scalp the micro-bacillus which he had described. He had also seen it in comedones and described it; therefore it appears as the bacillus of acne, an affection which is due, according to Sabouraud, to another micro-organism which is associated with it, the acne being in fact an epiphenomenon. Fourteen years ago Dr. Thin stated in our pages* that in the roots of hairs extracted from the margins of patches of alopecia areata, and between the root sheaths and the hair shafts he had found bodies coinciding in appearance with bacteria, and he believed that alopecia areata was caused by the development in the hairs of a minute organism or bacterium; carrying this belief into practice he showed that sulphur ointment thoroughly rubbed into the margins of the patches in many cases arrested the disease. The same observer had described† what he believed to be an organism that caused alopecia areata. He describes this organism in the following words:

"The difficulty of distinguishing . . . . granules from micrococci or from the spore forms of rod bacteria is so great that it was only when the characteeristic apearances of elongating spheroids, or small rod-shaped bodies containing spheroidal elements arranged linearly, or rod bacteria were observed, that the evidence of the presence of organisms was deemed conclusive . . . . . The result of the examination of a large number of hairs prepared by these methods has been to satisfy me that minute objects can be detected in them similar in size and form to those which I have recognized as organisms on the borders of freshly-extracted hairs, and preparations were obtained in which these objects were found in positions, and so arranged as to show that they were distinct from the rows and aggregations of minute granules which are found in healthy hairs. The objects referred to were seen either as round or as elongated rounded bodies."

The objects described by Dr. Thin as organisms do not seem to have been situated exactly in the same position as the micro-bacillus so carefully studied by Sabouraud. He considered that the bacterium described by him as "decalvans," penetrated downwards between the internal root sheath and the shaft towards the root of the hair. It then penetrated the hair substance, and as it multiplied it ascended upwards in the substance of the hair. Dr. Thin does not state that he had made any sections of the scalp, and did not succeed in staining the organisms, which renders an accurate comparison with such complete researches as Sabouraud's very difficult.—British Medical Journal.

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* British Medical Journal, 1882, ii, pp. 783 and 828.

† On Bacterium Decalvans: an Organism associated with the Destruction of the Hair in Alopecia Areata, by George Thin, M.D. *Proceedings of the Royal Society*, 1881, p. 217.